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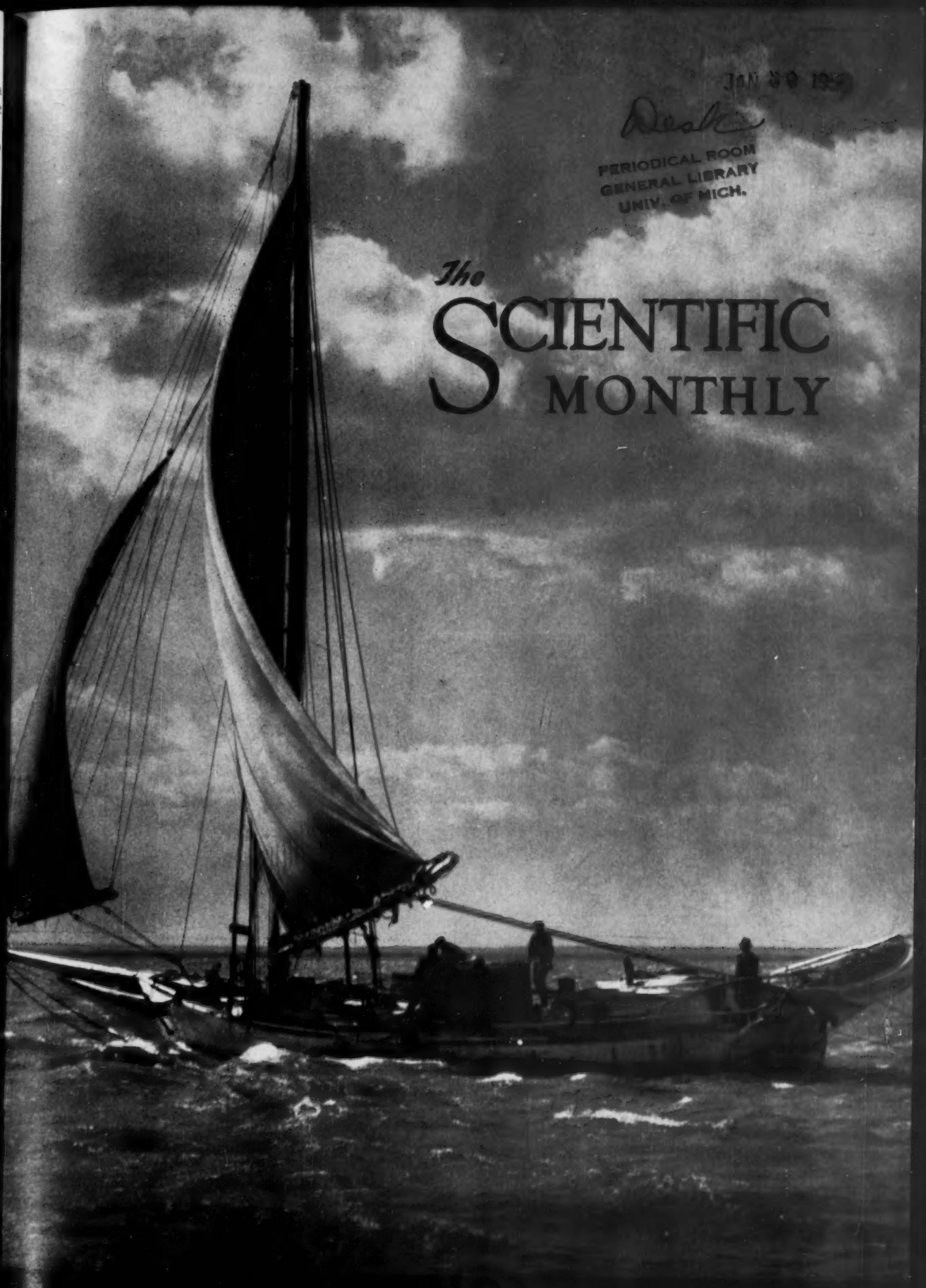

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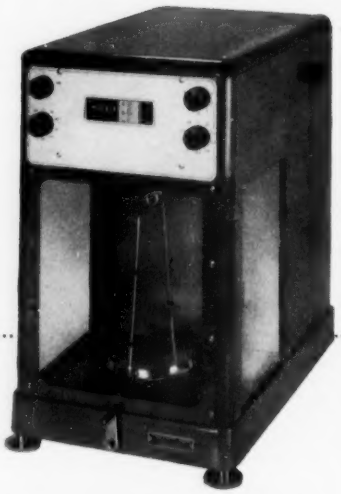

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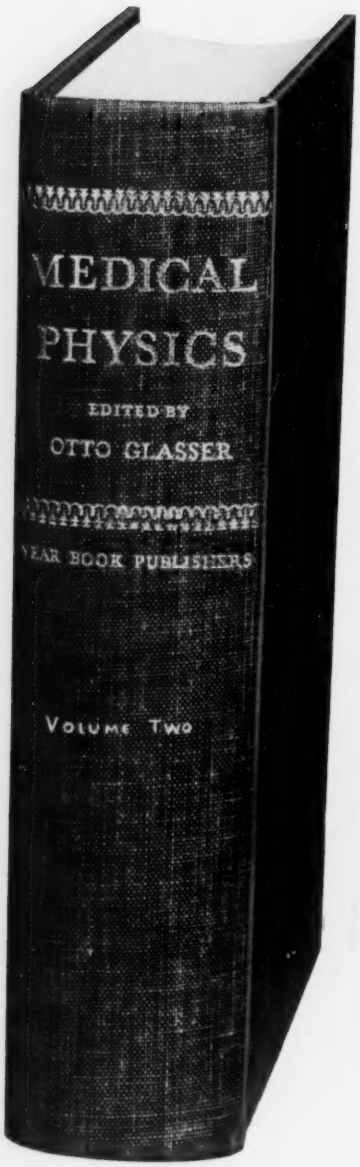

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THE
SCIENTIFIC MONTHLY

FEBRUARY 1950

Ecology—Geography—Ethnobotany

GEORGE F. CARTER

From 1934 to 1938 Dr. Carter (Ph.D., University of California, 1942) was curator of anthropology at the San Diego Museum. Since 1943 he has been at The Johns Hopkins University, where he is professor of human geography and chairman of The Isaiah Bowman School of Geography. His article is based on a paper presented in a symposium on "The Orientation of Ecology" at College Park, Maryland, September 1948.

WHEN H. H. Barrows in his presidential address before the Association of American Geographers defined geography as human ecology, he had in mind the tendency among many geographers to treat their subject as the study of the relation of man to his environment.[1] The definition did not please all segments of geographers, but it did serve to focus attention on the common ground between geography and ecology. But the study of the relationship of any organism to its environment leads one into a consideration of material drawn from many specialized branches of science. Consequently, anyone who is engaged in such study cannot be narrowly specialized in subject matter, at least not within the bounds of the usual disciplines.

Disciplines composed of habitual transgressors of the usual academic boundaries must, then, be unusually heterogeneous. Geography and ecology are both marked by diversity of subject matter and by strong centrifugal tendencies. Geography has at times been despairingly defined as "what geographers do." Perhaps the same has been said of ecology. Perhaps it was such thinking that led to a symposium on the orientation of ecology.

Geographers are currently in turmoil as to the nature, purpose, and methodology of their subject. Endless hours have been wasted, barrels of printers' ink and tons of paper have been used, and precious energy has been expended on attempts at closer definitions of the subject. A historical survey shows that such controversy has raged since the early nineteenth century.[2] Undoubtedly, and most unfortunately, it will continue to rage.

Neither geography nor ecology can be defined in terms of methodology or subject matter, for these will be determined by the problem being attacked by the individual worker. Both fields share an interest in holism, the reconstruction of complex wholes. In this day of great and increasing specialization, there is need for such efforts at a synthesis of knowledge. Reality, after all, is one great unity. It is only because man has a finite brain that for ease of treatment we have split reality into small chunks, conveniently labeled biology, geology, pedology, botany, and so forth. We should never lose sight of the fact that the academic boundaries are but man-made, artificial divisions of convenience. At best they do violence to the unity of reality.

There exists a constant need for sustained and intelligent effort to reconstitute reality; to study living things not as isolated particles, but as com-

plex organisms that exist in complex wholes in infinitely complex relationship to factors of the natural environment, which, in turn, is the result of a most intricate interaction of plants, animals, geology, soils, and climates. The need for such work is omnipresent.

A major issue is the question of the competence of any individual to achieve a holistic view of reality. In terms of a universal view the answer is surely negative. Even in terms of studies limited in time and place, it seems probable that no one man can comprehend all the ramifications of knowledge that could be brought to bear. Yet this seems no reason to reject the general attempt, in so far as we are able, to reconstruct reality in all its diversity. Even an incomplete understanding of a problem based on a multifactor approach is closer to reality than any single-factor study. A glance at the accomplishments that have resulted from multifactor studies of fish and game, for instance, shows that some investigators are capable of comprehending the principles of, and assembling data from, various fields to make noteworthy contributions to knowledge.

Few disciplines today are concerned with attempting such a holistic view. Geography and ecology, by so doing, are serving a worth-while function in an era of ever-increasing specialization. The correlation of the results of the work of specialists into more complex wholes has repeatedly led to great advances in our thinking. For example, Malin[3] has reviewed some of the effects on ecology of the successive additions of climatological, pedological, physiological, and morphological concepts to the study of the grasslands of America. Is this not justification enough for our existence? Need we waste precious time and energy in defining, delimiting, excluding? Let us rather seek knowledge and follow our interests wherever they may lead us. Let us welcome the maverick who fails to fit into narrowly defined academic fields, recognizing in him a kindred spirit who tends to view reality in broader terms and seeks to restore the interrelationships too frequently overlooked in this day of intense specialization.

Ethnobotany as a Field for Ecologists and Geographers

A field open to those unafraid to transgress academic boundaries lies in the no-man's-land between anthropology and botany and geography. This is the field of study of the relation between man and plants and animals. Here is an area where geographical, anthropological, and botanical knowledge can profitably be combined to give a fuller and more meaningful picture of the relation of man to his environment, the effect of man on plants, and the origins and routes of cultural impulses than can be gained by the use of one of these disciplines alone.

I shall here pay special attention to the plants used by man, including, of course, the domestic plants. The different groups of scientists concerned with man's use of plants have had quite distinct points of view. Ecologists, like geographers, have tended to limit their consideration of man in relation to plants to the historic picture and to modern man. The ecologist has leaned toward the nonagricultural plants; the geographer toward the agricultural ones. Ethnobotanists, on the other hand, have put special emphasis on primitive man and tended to include all plants actually harvested in some way—for example, whether for food or for house thatching.

It is probable that ecologists and geographers, too, could profitably follow the ethnobotanist and study all the plants affected by man's presence, and that all would benefit by considering a longer span of time than has been customary.

Man, Fire, and Time

Primitive man's use of fire to drive animals must have been fully as important in the past (if not more so) as fire is today in modifying vegetation. To what extent does the modern scene reflect that past? A major question, of course, is how long man has been active as a plant-modifying agent. In the Old World we have evidence for the presence of man, *Homo,* at the opening of the Pleistocene, perhaps a million years ago. Modern man, *Homo sapiens,* is present from mid-Pleistocene times on. Fire-using by man is certain from mid-Pleistocene times and may well have started earlier. Since man has never been noted for care in handling fire, and since primitive man is known long to have used fire for the purposes of driving game and improving the range, it would appear that man-set fires may well have had an influence in the Old World dating back for half a million years, and perhaps longer.

But so would many other effects of man's activities. Early man was an unspecialized gatherer. In a sense he was probably not a hunter. Rodents and reptiles and small mammals that crossed his path were undoubtedly "gathered in." But quite likely his early mainstays were fruits and buds and bulbs and seeds. Would not constant harvesting of selected species over a period of a million years have profound effects on such plant life? To what extent have ecologists studied the habits of primitive

peoples for clues to the type of effect man in the Old World may have had upon his surrounding plant and animal world over the past million years?

Here in the Americas we are, of course, dealing with no such immensity of time. Until recently it has been fashionable to consider that man has been present in America but 5–10,000 years, and that he entered with a near-neolithic culture, though lacking agriculture. Today there is grudging acknowledgment that man may have been present for 15,000 years, or possibly 25,000 years, and that the cultural level of the earliest immigrants could better be likened to the paleolithic of Europe. Even these estimates are of terms of years so appreciable that men could markedly have affected our plant and animal assemblages. And yet such estimates probably fall considerably short of the mark. In our now-arid Southwest, evidence of man's presence on the shores of vanished lakes is common. Although attempts have been made to assign these to relatively recent times on the grounds of assumed post-glacial pluvial periods, no convincing climatological mechanism has yet been put forward to account for such a post-glacial pluvial. The evidence is more in favor of a Pleistocene age for these features, and a pluvial period coinciding with the last glacial period seems indicated.[4]

Field work during the past two summers on the seacoast of San Diego County, in southern California,[5] has produced evidence that man lived on this coast during the time the sea last stood low in relation to the land. This low sea stand is thought to be correlated with the removal of water from the sea and its locking up in the form of ice on the lands during Wisconsin glacial times. The evidence is associated with soils now perhaps climatically out of place and with evidence for a complex history of erosion and alluviation, with intervening intervals of sufficient length for the development of mature soil profiles. A time span equalling Wisconsin glacial time, or about 100,000 years, seems indicated. And there are tenuous bits of evidence to show that this figure may represent but half the time allowable for man's presence in America.

If man set fires annually in California for 100,000 years, what would be the effect on the vegetation, hence on the soils, and consequently on the plant and animal life? To what extent is it meaningful to try to interpret these features without considering man's role in their development? May not the widespread grasslands of central California be the result of such long-continued activity? Although I am aware that the relation of annual burning to the formation of grasslands is a difficult and controversial question, it is inconceivable to me that annual fires over any appreciable fraction of 100,000 years would fail to modify the vegetation markedly. However, here I am only trying to call attention to the fact that an ecologist, in pursuing a full understanding of how the present situation arose, should be aware of the facts that man may have been in California and elsewhere in the Americas far in excess of 100,000 years, and that man-induced fires may have been playing a role in the development of the ecology of California over this very appreciable period of time.

That fire may have played a significant role in determining the vegetative type in parts of California seems indicated by the Torrey pines. These rare trees now occupy the edge of a sandstone mesa. The soils are very thin and poor, and the trees are exposed to extreme wind action and excessive erosion. They seem to cling to the mesa almost despite the environment, rather than because of it. That there is nothing in this soil or microclimate critical for the support of the tree is suggested by the fact that Torrey pines grow elsewhere in California and the world. Where they have been planted elsewhere in the vicinity of San Diego, they have demonstrated their ability to thrive on unirrigated land in competition with the chaparral. Why, then, are they limited to the northwestern projecting tip of the Torrey Pines Mesa?

A look at this projecting mesa tip on a topographic map gives a clue. The ocean lies to the west, and wet marsh and open slough lie to the north and east. Fire could reach the tree-bearing area only from the south and by moving directly against the prevailing wind of the dry season. The evidence suggests that the pines survived in a fire-protected area, and that if protected from fires, they could today well maintain themselves over much wider areas than their present restricted habitat.

If we are correct in thinking that the Torrey pines may have been limited to their present area by fire, what might have been the source of the fires? Coastal southern California is one of the most lightning-free areas in the United States. Such lightning storms as do occur usually accompany sharp cold fronts following a winter cyclonic rain. This would be the least likely time for fires to start. For frequent fires during the dry season, man would be the most likely agent. If man either deliberately or accidentally allowed fires to run through this country, he might well have converted it from a Torrey pines forest to the chaparral land we know today. The relict, fire-protected position of the modern Torrey pines, together

with their demonstrated ability to grow well over wider areas, would fit into such a theory. There are other "islands" of pines along the west coast of America. Might not a study of their locations and their climatic needs, in the light of the above, be of ecologic, geographic, and economic interest?

Other Man and Plant Relations

The California Indians were energetic gatherers of wild grass seeds. Their methods and utensils included the seed beater and seed tray. Their technique was to walk through the grasslands while striking the seed heads toward their baskets with the tennis-racket-like seed beater. By this means a quantity of seeds could be gathered in a day. But what must the effect have been on the grasses?

Such an annual harvesting must have furnished a nearly ideal seed dispersal for the grasses selected for food by the Indians. For surely not all the seeds were caught in the basket even by the most skillful Indian woman. If this process went on over a period of 100,000 years, or even 50,000 years, might there not have been selection for a grass that would hold its grain until shattered by the blow of the seed beater, which would assure it greater dispersion than could be expected in nature? Suppose some of the California grasses had been so adapted, what would have been the result of the sudden disappearance of Indians after 1800 and the sudden loss of a superior means of seed dispersal? Is the success of the Mediterranean oat in invading the grasslands of California at this time causally related to some such sequence?

These are but questions and hypotheses. We do not have the answers. But here is a field of inquiry where ecologists could contribute much of interest to geographers and anthropologists, and perhaps the findings would be not insignificant for ecology itself. For instance, one of the characteristics of our modern domestic grasses, such as wheat, barley, oats, and rye, is the loss of a fragile seed head and a consequent holding together of the seeds rather than a dispersal of them. There has been considerable speculation concerning the origin of this trait which is so inimical to the plant. May it not have developed through some such set of circumstances as related above? If man had come to specialize in gathering one grass more than others—and there are many examples among primitive peoples of such a tendency to concentrate on certain plants in their environment—the plant in question may, in turn, have become more dependent on man. The ultimate conclusion of this sequence is the purposeful propagation of the plant—that is, agriculture.

The fundamental question raised here is this: Can man use a plant over a period of 50,000 or 100,000 years without modifying the plant? Or, to state it in another way, is it not to be expected that man's annual harvesting of wild plants should become a factor affecting their dispersal and survival and reproduction rates, and hence a selective factor of some importance?

Study of the wild plants used by primitive peoples might aid in understanding the distribution of some of those plants. Recently, attention has been called to the use of *Hevea* for food by the primitive peoples of the Amazon as a factor that probably influenced the distribution and variation of the trees and probably had effects on its ultimate utilization as a source of rubber.[6] In this case, primitive man's use of the tree resulted in the establishment of many separate clones around native clearings. From the resulting high variability came great opportunities for selection. This is both an interesting and meaningful insight into man's influence on the distribution and evolution of a "wild" tree. Our knowledge of it resulted from the combination of ethnological, geographical, and botanical data. Through merging such information from these fields there are many significant findings to be made. Among such not insignificant facts is the recognition that many of the trees long treated as wild have been under human influence and modification for a great period of time.

The distribution of the peach palm (*Guilielma gasipaes*), from the eastern Andean foothills through the Amazon basin to the Orinoco basin and thence into the Caribbean, has also been suggested as perhaps due to man's use of this tree. How, then, are botanists and ecologists to know what is a "natural" distribution or a "natural plant formation" unless they have checked the plant in question against the plant uses of the peoples of the area, both past and present, in so far as they are known?

As an indication of the number of plants used by man whose distribution and evolution have probably been influenced by him, I give in Table 1 a list of plants cultivated or semicultivated within what Vavilov[7] has designated "The South Mexican and Central-American Center of Origin, Including the Antilles." It is important to note that this list covers but one part of America and is very likely incomplete.

Many years ago O. F. Cook pointed out that the tree which is now our source of chicle for chewing gum was to the Maya Indians of such importance for nuts that often the trees were left standing when a new agricultural clearing was made. When,

TABLE 1

GRAINS

Zea mays L.—maize
Phaseolus vulgaris L. Savi—bean
P. multiflorus Willd.—multiflorus bean
P. lunatus L. gr. *microspermus*—Lima bean
P. acutifolius A. Gray var. *latifolius* Freeman—tepary bean
Canavalia ensiformis DC.
Chenopodium nuttaliae Saff., *C. ambrosioides* L.
Amaranthus paniculatus L. var. *leucocarpus* Saff.—amaranth

GOURDS

Cucurbita ficifolia Bouche (*C. melanosperma* A. Braunn)—fig-leaved pumpkin
C. moscata Duch.—squash or pumpkin
C. mixta Pang.
Sechium edule Swartz.—chayote
Polakowskia tacaco Pittier (Costa Rica)
Sicana odorifera Naud.

ROOTS AND BULBS

Pachyrrhizus tuberosus Spreng. (*Cacara edulis* Kuntze)
Ipomoea batatas Poiret—sweet potato
Maranta arundinacea L.—arrowroot (Antilles)

SPICE PLANTS

Capsicum annuum L.—Cayenne pepper
C. frutescens Will.—many-leaved pepper

FIBER PLANTS

Gossypium hirsutum L.—upland cotton
G. purpurascens Poir.—Bourbon cotton
Agave sisalana Perrine—henequen; *A. ixtli* Karn.—Ichtli agave

FRUITS

Opuntia spp.—tuna
Anoma cherimolia Mill. (possible secondary center); *A. reticulata* L., *A. squamosa* L., *A. muricata* L., *A. purpurea* Moc. and Sesse, *A. cinerea* Dun., *A. diversifolia* Safford, *A. glabra* L.
Sapota achras Miller, *Sapota sapotilla* Coville—sapodilla
Casimiroa edulis La Llave—white sapote
Calocarpum mammosum (L.) Pierre—mamet
C. viride Pittier (*Achradelpha viridis* Cook)
Lucuma salicifolia H.B.K.—yellow sapote
Carica papaya K.—papaya
Persea schiedeana Nees and *P. americana* Mill. (*P. gratissima* Gaertn.)—avocado
Psidium guayava L.—guava
P. friedrichtschalianum (Berg) Niedenzu, *P. sartorianum* (Berg) Niedenzu
Spondias mombin L. and *S. purpurea* L.—Mexican plum
Crataegus mexicana Moc. and Sesse, *C. stipulosa* Stend.—*tojocote*
Diospyros ebenaster Retz.—black sapote
Chrysophyllum cainito L.—cainoto (particularly in the Antilles, in Jamaica, Panama, wild and in cultivation)
Anacardium occidentale L. (Antilles, Panama)
Prunus serotina Ehrhart (*P. capollin* DC.)—capulin (mainly wild)

MISCELLANEOUS

Agave strovirens Karw.—pulque agave, maguey
Cereus spp.—cactus for hedges
Nopalea coccinellifera Mill.—cochineal cactus
Tigridia pavonia Ker. Cawl.—cacomite (mainly wild)
Physalis aequata Jacq.—Mexican tomato
Lycopersicum cerasiforme Dun.—tomato
Salvia chia Fernald—chia (for fats)
Theobroma cacao L.—cacao
Bixa orellana L.—achiote (coloring and spice)
Nicotiana rustica L.—tobacco

in a year or two, the clearing was abandoned, the chicle trees had the greatest opportunity to fill up the clearing. It is probably to such activity, extending over thousands of years, that the chicle forest of Yucatan owes its existence.

Recently, E. C. Higbee[8] has reviewed this evidence affirmatively and pointed to a similar situation in the United States. Nut-bearing trees comprised a surprisingly high proportion of the Eastern hardwood forests. Here, too, man for a period upward of two thousand years has practiced an agricultural system built upon a long rotation, with forest cover used to restore the soil fertility to the clearings abandoned after a few years' cropping. The Eastern Indians made much use of the nut trees and quite probably left them standing whenever it was feasible. Their influence over the years must have affected the make-up of the woodlands.

But such practices were not limited to Yucatan and the Eastern United States. They were practiced in tropical South America and Middle America, and in humid Africa and Asia. In these regions, too, man must long have been modifying the make-up of the forest cover. Just what do we mean, then, when we refer to "the original condition"?

Of the wild cucurbits, one has a particularly wide distribution. *C. foetidissima* is to be found from Guanajuato, Central Mexico, to Kansas and Nebraska and to the coast of Southern California. In 1944, when reviewing the distribution of the cucurbits, it seemed to me that a possible clue to *C. foetidissima's* wide range is to be found in the fact that primitive Americans ate its seeds.

Is *C. foetidissima* a domesticated plant? It has never been considered so. Its distribution has been more or less assumed to be natural. It seems quite likely, however, that its distribution is man-made and its success in entering so wide a range of ecological niches a thing to be explained in that way. M. R. Gilmore pointed out similar distributions of "wild plants" a number of years ago,[9] naming, for example, sweet flag (*Acorus calamus*), black walnut (*Juglans nigra*), and tobacco (*Nicotiana rustica*).

Man and Domestic Plants

The domestic cucurbits also pose interesting questions. There are three distinct species: *C. pepo* in North America, *C. moschata* in Middle America, and *C. maxima* in South America. Each has a putative wild ancestor in its central area. Are these, then, separate domestications? Or is this a natural degree of divergence within a species under domestication? From geographical, ethnographical, and genetic grounds, we have argued elsewhere

that the divergence is best explained on the basis of separate domestication of these species.

We enter here the whole field of domestic plants and encounter the problems of the origin of agriculture and the diffusion of domestic plants. We know exceedingly little about time and place of origin in agriculture, the processes of domestication of plants, and the routes and methods of diffusion of domestic plants. Yet all these are in part ecological problems. Under what circumstances does the relationship between man and his plant world become such that certain plants are thereafter systematically propagated and selected for desirable properties? Does plant domestication have a plural or a single origin?

Until very recently it has been axiomatic that the Old and the New World agricultures were entirely separate, having no plants in common. This was in spite of the works of O. F. Cook, published over forty years ago, which indicated the improbability of this thesis.[10] Recently, a very strong case for the Asiatic domestication of cotton and its trans-Pacific transportation has been made.[11] The presence of the bottle gourd (*Lagenaria siceraria*), the coconut, and other reputedly Asiatic plants in early agricultural levels in America reopens the question of the separateness of Old World and New World agricultures.

These problems as to which are Asiatic and which are American plants are important and interesting, but perhaps less ecological than others. Surely, however, the environmental situation that gives rise to the domestication of plants is of ecological interest. The Russians, under Vavilov, began the assemblage of data that would help us to understand such a problem. Their work in the New World was rapid, and presumably many of their surveys in other parts of the world were done also in some haste. Yet they showed that there were several centers in America, each containing a characteristic group of domestic plants. The grouping of the plants was of interest. Some centers are characterized more by grain plants, others more by tuberous plants. What is the significance of this? Were tuberous plants originally lacking in the one and grain plants in the other? Would not a study of the plant, land, climate, and human relationships in some of these areas, to see what, if any, common denominator they might hold, be a problem worthy of an ecologist's attention? Other scientists, particularly anthropologists, would be interested in the ecologists' opinion on the evidence for single or plural centers of origin of plant domestication within America.

For obscure reasons, the domestic plants have received less study than their importance in our economy and their intrinsic interest would seem to warrant. Not only do we still not know the origin of maize—it is probably not a Mexican domestication, probably not a descendant of *teosinte,* and it may just possibly be an Asiatic domestic—but we are exceedingly ignorant of how, when, and where our modern commercial varieties arose. Not only does this mean that we lack information of great interest as to the times, places, and routes of diffusion in the prehistoric and early historic times, but we lack the grounds for judging adequately the corn potential of this and other lands.

The work of Anderson[12] and his colleagues is only now clarifying the origin of our cornbelt dent corns. These prove to be a compound of Northern Flints for hardiness and Southern Dents for other characters. Such studies tend to show that the corn plants now in use are a haphazard combination of a few of the limited strains available in the colonial United States. Yet the pattern is now so established that radical changes of varieties would be difficult because the farmer has an ideal as to plant size and shape, and the consumer an established ideal as to grain size, color, shape, and weight. Few of these factors are meaningful; yet their establishment as a norm has led to the development of a relatively static plant situation, and warps our judgment of man in relation to his environment in terms of corn. And the same can be said of nearly all our domestic plants.

We limit ourselves in corn-growing by trying to carry this established "normal" in corn into other areas. We tried to carry dent corn into the Dakotas and failed. We have tried repeatedly to establish dent corn among the Indians of the Southwest and failed. In this last instance, we have repeated an experiment that was tried in the same area, and likewise failed, a thousand years ago.

In our hot and arid Southwest an interesting situation exists as to beans. The Indian people of the low hot desert country have for about 2,000 years grown a particular species of bean (the tepary bean, *Phaseolus acutifolius*) which is admirably suited to great heat, little water, and high alkalinity. The Indian people of the plateau country grow a different species of bean, our common *Phaseolus vulgaris,* which is slower-sprouting, more alkali-sensitive, and subject to premature abscission of the flowers in extreme heat. On much of the plateau area and in the Rio Grande Valley, the tepary bean of the desert dweller would be ecologically better suited for growth. But because of the force of custom, the *P. vulgaris* bean is grown by modern farmers instead.

Nor may we smile at the vagaries of primitive peoples who refuse to grow the plants best suited to their ecological situation. The tepary bean, despite its many advantages—for in addition to its adaptation to hot climates it makes a nutritious forage and hay, cooks in much shorter time than *Phaseolus vulgaris* beans, and is less gas-forming—is not used today by peoples of our culture. Nor does it seem to be known in those other parts of the world where it would appear to fit into what is for man a difficult environment—for example, in arid Africa, North Australia, India, or the Near East. Any realistic assessment of the ecology of the peoples of the hot and arid lands would be incomplete if it failed to consider the possible role of this useful plant in such areas.

Similarly, our view of the problem of the settlement of the wet tropics is myopic because we have failed to integrate knowledge from separate fields. One of the needs in tropical rainy lands is tree culture. Trees are the natural vegetative cover for the area. They provide an unbroken soil cover and maintain the biological cycle of humus to plant to humus. They tend to supply a continuous crop and thereby take fuller advantage of the unbroken growing season. Trees that supply vegetables, fruit, and fibers of great variety are known in various tropical areas and have been used, and more or less modified, by native peoples. Witness, for example, the formidable and probably incomplete list from a single part of America where the native peoples have paid less attention to tree crops than did the peoples of Southeast Asia.

An ethnobotanical survey of useful tree crops of the world, followed by an attempt to construct balanced tree-crop agricultures for tropical lands, might prove to be one means of rehabilitating some of these lands, although, of course, one would even then be faced with the difficult task of persuading the native peoples to adopt the new ways. The need for some such tree-crop agriculture in tropical rainy Latin America is desperate. The economist himself would never think of such a project. The anthropologist, though he might record the plants used by native peoples, has up to now shown little interest in the possible world-wide utility of such plants. The narrowly specialized botanist is more than likely to be content with classifying the tropical trees and cataloguing their growth requirements. From whom, then, is to come the integration of knowledge that might bring these "mere facts" into a meaningful whole? J. Russell Smith,[13] an ecologically minded geographer, has led off with a powerful book on tree crops as a permanent agriculture. There is need for more men of breadth of approach and with synthetic, integrative minds to tackle this and similar problems. Might this not be a proper orientation for ecology?

Finally, such considerations as those above lead us to the discovery that we have made a significant and probably very dangerous change in our outlook on domestic plants. If we reason correctly, our present crops are the result of very long processes of plant modification which probably began quite unconsciously. Even the conscious modification of plants has been under way for great periods of time. Our domestic plants, where datable, first appeared in fully developed form about 5,000 years ago. At that time they were virtually as far from their possible wild ancestors as they are today. It seems likely, therefore, that by then they had already been long under human modification. A time for the beginning of the conscious propagation, selection, and modification of plants as early as ten or even fifteen thousand years ago does not seem unreasonable. And back of that time lies the immensely longer period of unconscious human influence upon plants, influence of the type suggested by the relationship between the California Indians and their grassland.

Our heritage from these successive ages has been a diverse assemblage of plants, each of which represents millennia of effort. Among noncommercial folk each variety is cherished for some special quality. The relationship between man and plant is felt to be very personal. Variety is considered good, and that one individual's beans may be different from those of his neighbor is thought right and natural. The result has been to establish a vast genetic reservoir filled with an almost infinite range of variations. Folk wisdom has thus maintained in its plant treasures the safeguard of diversity. Should blight strike any crop, someone would surely have a resistant strain.

We have drawn from this rich stock for all our commercial strains. Today we still send our plant explorers to "primitive peoples" to collect strains of plants with qualities of use to us. Everyone, indeed, should be familiar with the story and the significance of Acala cotton and Hopi Lima beans. The former, originated by Mexican peasant folk, has made possible our arid-land cotton agriculture. The latter, because of its nematode resistance, has saved the entire Lima-bean industry in California. The significance of such facts is that without a great diversity of crop material from which we may draw resistant or specialized varieties to meet future unforeseeable needs, we face imminent ruin.

What, then, are we doing to ward it off? We continue to standardize our crops, and as we draw

the peasant folk of the world into our commercial orbit, we demand of them that they do likewise. The result, the abandonment of their characteristic crops by these simple folk, is a steady wasting of the varied heritage of the past. This is not visible to the economist and has not been discovered by many agronomists or botanists. It lies beyond the bounds of consideration by anthropologist or narrow specialist. Notwithstanding, it is a wastage of a basic resource that may be second only to the accelerated soil wastage of our times.

Is it not significant that the revelation of our dangerous course should spring from the integrative studies of workers in ecologically minded geographical schools, little restricted by traditional academic boundaries? Is not the field of man and his relations to his useful plants a field that needs and deserves the ecologist's attention? It is to be hoped that ecologists, in orienting themselves, will include this attractive field. One can also hope that the orientation of ecology will be outward, that it will welcome and encourage those men whose work falls between the narrowly defined academic disciplines.

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NOW I discovered that nothing is of such benefit to the human race as the discovery of and devotion to new truths and arts by which the life of men is cultivated . . . and above all if a man should bring forth no particular invention, even though quite useful, but were to kindle a light in nature which from its very beginning would illumine the confines which hold within their grasp facts already discovered, and once raised a lot would straightway lay open and bring into view the most hidden things, that man seemed to me to be the propagator of man's empire over the universe . . .

—Francis Bacon, Works VI, 446—50.

The Strange Case of the Coca Leaf

CARLOS GUTIERREZ-NORIEGA and VICTOR WOLFGANG VON HAGEN

A member of the faculty of pharmacology of the University of San Marcos in Lima, Dr. Gutierrez-Noriega has studied coca clinically for many years. A list of his publications on the subject includes the paper "El Cocaismo y la Alimentacion el Peru" (Anales de la Facultad de Medicina, *Lima, 1948,* 31). *He is an Academician of the National Academy of Exact Sciences (Peru). Mr. von Hagen has conducted five major expeditions to South America, where he has made an extensive study of the cultural history of coca. He is the author of twenty-one books on various phases of Latin-American culture, the latest being* Ecuador and the Galápagos Islands *(1949).*

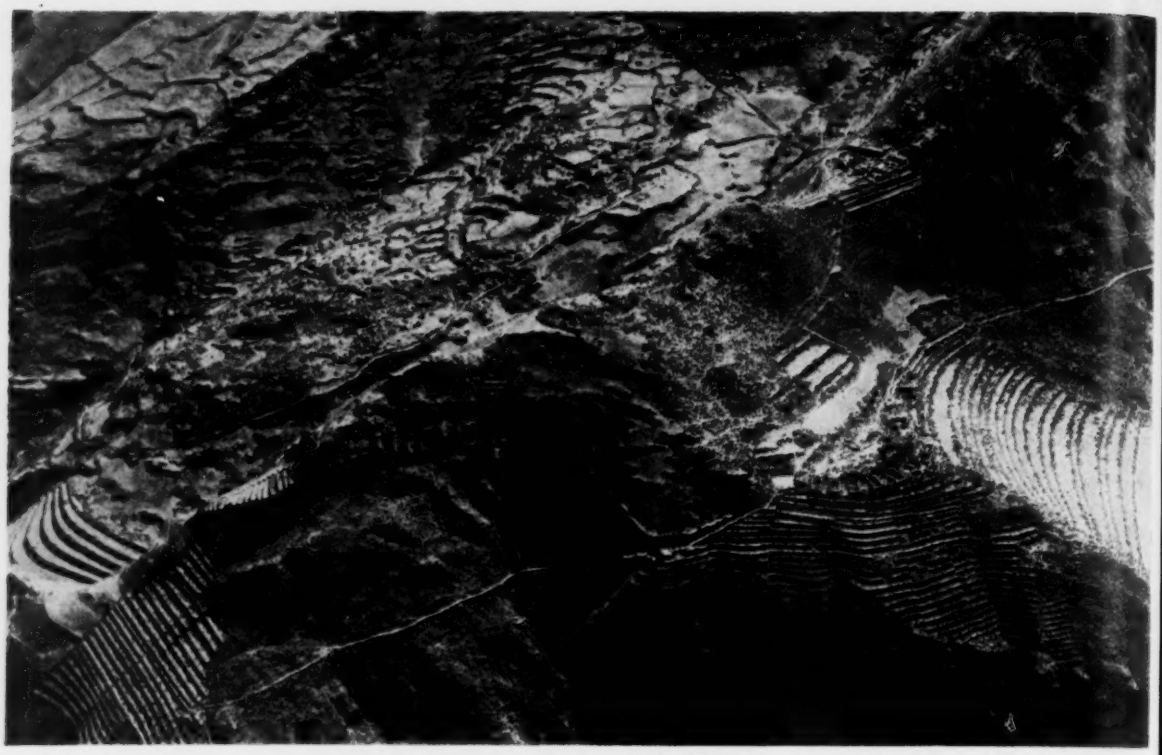
COCA, a South American shrub (*Erythroxylum coca*), whose delicate tealike leaves yield cocaine, has had a strange and fascinating history. Originating in the Amazonic jungles centuries ago, coca leaves were discovered—how, we do not know—to produce, when chewed with lime, an exhilarating sensation, a "lift," a feeling of lightness, and an increase of energy. Whenever Indians trekked the South American jungles, it was "good custom" to carry along a bundle of dry coca leaves which, when chewed and the juice swallowed, made hunger less insistent and gave the body energy to endure fatigue. This coca habit, now believed to have had its origin among the tribes of the central Amazon, eventually broke out of its jungle-bound habitat and was carried by trade into the Andes. There, among the mountain tribes, coca chewing took fierce hold. By the time the Inca civilization burst out in its full glory in the tenth century, the coca leaf had already become an Andean institution. However, since the coca leaf aroused strange sensations, it was called a "divine plant," and its use was limited to members of the Inca aristocracy.

Statistics regarding the present-day chewing of the coca leaf and its accompanying scourge, the cocaine habit, as released by the United Nations Committee on Narcotics, are sufficiently terrifying. Bolivia produces more than 5,000,000 pounds of coca leaves annually, a million pounds of which are shipped to Argentina (a trade which amounts to 18,897,680 bolivianos!) to be used by harvest hands, who chew the leaf while gathering in the crops. Bolivia consumes the residue of her production, and, the demand being more than the supply, she finds it necessary at times to import an additional tonnage from Peru. In eight years, coca production has increased more than 10,000,000 bolivianos, until it is now so important an element in Bolivian economy (31,890,278 bolivianos annually) that the government frankly admits in answering a United Nations questionnaire that the loss of the plant or a curtailment of its production would pose a *"real problem"* to Bolivia and Argentina.

Peru, the center of coca production (and consumption), is faced with an enormous sociological issue arising out of coca chewing. Out of a population of 7,600,000 inhabitants, more than 3,000,000 are *coqueros* ("coca chewers"), and Peru is producing over 17,000,000 pounds of coca leaves annually. This means, on the basis of 1.2 per cent yield of cocaine from the coca leaves, that Peru's population is consuming approximately 85,000 pounds of cocaine each year!

On the basis of these terrifying statistics, the government of Peru decided to bring the matter into the open before the United Nations Economic and Social Council; their permanent delegate asked the council to send a corps of experts to Peru to study the matter at firsthand. Now there is in the field, in Peru and Bolivia, working at the dizzy heights of the lofty *puna,* an investigating committee of the United Nations, composed of specialists, making inquiry into the physiological and psychological aspects of coca chewing.

Coca claims more addicts, per capita, than any other narcotic-yielding plant; 15,000,000 South



The face of Peru. It is on terrain such as this at Pisac, high, treeless, where the pallid sun barely warms the frigid air and where the motif of life is bleakness, that coca chewing is most prevalent. (*Robert Shippee, from Frederick Farnam Associates, Inc.*)

Americans, mostly Indians and cholos, ranging geographically over half the continent from Argentina to Colombia, are addicted to the cocaine-yielding leaf. Half of Bolivia's heaven-high population are *coqueros,* 3,000,000 in Peru are enslaved by it, and in Colombia whole populations are addicted to it and in the process of slow degeneration from its cumulative effects. In Bolivia and Peru all work, all physical activity, is geared to this narcotic-yielding leaf. Miners descending into Bolivia's tin mines do so with wads of coca leaves stuffed into their cheeks; were they suddenly to be deprived of it, tin production would stop and Bolivia would disintegrate into chaos; a good portion of Argentina's harvests are gathered by coca-chewing farm hands. As for Peru, no work of any kind can be started in the central Andes without the daily ration of coca leaves. Not many years after the conquest, the learned administrator Juan Matienzo de Peralta made a pertinent observation: "If there were no coca there would be no Peru." What was true then is equally true today. This addiction to the coca leaf by Peru's millions has been so much a part of the economic fabric of Peru that no government since the conquest has had the courage to proscribe it.

Coca, not to be confused with *cacao,* which is "chocolate," or coco, which is the "coconut," is cultivated in the lush valleys of the lower Andes in Peru and Bolivia on terraces on the sides of the mountains, far distant from the point of consumption. Some of these coca plantations are so ancient that they go back to pre-Inca times. Coca is as old as Peru. The leaves of this "divine plant" have been found in pre-Inca graves dating back to the ninth century, in the same small woven bag as is carried by modern addicts; here too is the same lime (*llipta*) used in chewing today. There is, however, one great difference; in the time of the Incas coca was restricted to the ruling classes; it was denied to the common Indian, and thus its addiction was limited. The conquest of Peru by the Spaniards changed all this; Inca society disintegrated under the shock of the conquest and opened up to the Indian many things he had been denied under Inca rule. One of these was the chewing of coca, and the habit spread like wind-blown fire. Coca planting was encouraged by the Spanish colonists, and the Indian, forced to work in the mines, or to go on backbreaking expeditions into tropic lands, found that coca chewing numbed his perceptions and gave him endurance. Within a few years after the conquest, more than 2,000 Spaniards were connected in one form or another

with coca production; the much-traveled Pedro Cieza de Leon, who penned the finest of the conquest chronicles, wrote, "There are some persons in Spain who are rich from the produce of this coca, having traded with it, sold and resold it in Indian markets." By 1548, the Indian workers in the silver mines of Potosi, high up in the Bolivian Andes, were consuming 100,000 *cestas* of coca leaf yearly. "It seems almost fabulous," wrote José de Acosta, a contemporary chronicler, "but in truth the trafficke of Coca at Potosi doth yearly amount to above a half million piastres." Soon coca chewing engulfed whole populations in the Andes, and the habit was alternately extolled and condemned. The Church, although it drew tithes from coca production—("The greater part of the revenue of the Bishops of and Canons of the Cathedral of Cusco is derived from the tithes of the coca leaves," wrote Garcilasco de la Vega)—denounced the addiction to coca as a diabolical illusion, "the chewing of which Indians suppose gives them strength," and so made prohibition against it; it was in vain. The Viceroy, the Marques de Cañete, dictated a ukase to restrict its production in 1567; it was ignored. And while coca chewing spread its cancerous growth throughout the Andes, jumping over the racial lines of the Indians, reaching out to the cholo half-castes, up even into white society, the magic of the coca leaf was heatedly discussed by European savants. Dr. Nicholas Monardes thought he had found the elixir of life, and he extolled the coca leaf in his book translated as *Joyful News Out of the New Founde World*; whole European populations, it was believed, could be sustained by merely chewing the coca leaf; and as late as 1814 the *Gentleman's Magazine* was begging Sir Humphry Davy to undertake experiments with coca so it could be used as a "temporary antifamine or substitute for food, so that people could live a month, now and then, without eating. . . ."

Workers lining up for their morning ration of coca. In Peru, everything is conditioned by the coca leaf. (*W. H. Hodge.*)

Whether coca chewing is harmful or whether it is beneficial has been heatedly argued for four centuries, but not until Gutierrez-Noriega, of the University of San Marcos in Lima, undertook to study the matter in situ have we had a detailed scientific study of coca's immediate effects on the physiological and mental processes. The following portion of this article, a summation of Gutierrez-Noriega's years of research, gives the first sustained study of it in English. This subject, because of the interest aroused by the United Nations Field Study, will become in the next few months one of intense interest.

The Physiological Effects of Coca Chewing

The annual consumption of coca during the three centuries of the Spanish domination of Peru and Bolivia may be estimated at 6,000,000 kg per year. If we take into account the fact that the Andean population of those two countries has chewed this enormous amount of coca during all these centuries, the total amount of coca leaves consumed in that period may be calculated as 40,000,000,000 kg, and the amount of alkaloids extracted and ingested would be no less than 20,000,000 kg. An exhaustive inquiry into coca addiction, on the basis of this information, must take into account the historical background. The influence of the drug through many generations may have some importance as a creative factor in psychological disturbances and racial degeneration.

The daily dose of dried coca leaves chewed by addicts varies between 10 and 100 gm, with 30 gm as a mean value. In some few cases the daily dose varies from 200 to 500 gm. The product is consumed through an intensive chewing, and the saliva serves as the extracting agent of the coca alkaloids. About 50 or 70 per cent of the chewed leaves is swallowed, and the rest is thrown away. The coca addicts chew the leaves two or three times daily, in particular before their work. The amount of alkaloids extracted and absorbed during a period of coca chewing is, as a mean value, 86 per cent of the total amount of alkaloids contained in coca leaves. Sharp assumes that the leaves might be only half-exhausted, thereby arriving at an estimate of 194–

The coca habit is as old as Peru. This Mochica vase (*circa* 1350) shows an Indian resident of Chan-Chan, his left cheek swollen with coca leaf, dipping his lime stick into the *poporo*. (*A. Guillen.*)

324 mg of cocaine daily. Bignon, who made a study of coca leaves, considered that coca contains an average of 0.5–0.6 per cent, arriving at a figure for the daily dose of 300–400 mg.

These figures, however, were not drawn from the chemical analysis of coca *bolos,* the masticated leaves thrown away by the chewers. The chemical analysis of this coca material was made for the first time in my laboratory, where several hundred coca wads were analyzed. This study has shown that the individual daily dose of cocaine alkaloids ingested (if 30 gm of coca leaves are chewed) varies from 80 to 400 mg, with 180 mg as a mean value. The mean dose of cocaine per kilogram of body weight is, more or less, 2.5. mg, but 6 mg per kg is the maximum dose in some cases. It is difficult to believe that such a large dose, ingested every day during almost an entire lifetime, can be harmless. A simpler calculation than those preceding may be attempted by considering only the leaves.

Coca leaves are masticated with lime, or with the alkaline ashes of certain plants. The addition of alkaline substances is sometimes explained as being necessary to extract the alkaloid. I have found that alkaline substances are not indispensable to set free the alkaloids from the leaves, although it has been found that the percentage of the extraction is slightly greater when the leaves are chewed with lime. Much more important is the effect of these substances upon the physiological action of the drug. For that reason, if the coca addict is obliged to chew the leaves without lime, he requires a slight increase of his customary dose of coca leaves. We are not sure whether this potentiation of the effects of coca alkaloids by alkaline substances is the result of increased absorption by the gastrointestinal tract, or the result of an increased permeability of the central neurons, or to a combination of both factors. Recent experiments on rats and dogs, which showed that cocaine toxicity increases when the drug is injected after a previous administration of alkaline substances, point to the reliability of the last hypothesis. Comparisons were made between the physiological changes in coca chewers taking the drug, with and without their customary alkaline substances. Stronger reactions were always obtained in the first experimental conditions.

As an example of the potentiating action of alkalines upon coca alkaloids, there is the case of a non-addicted subject who chewed 20 gm of coca leaves without alkalines. He did not show any serious alteration in this experiment. A few days later, he chewed the same dose with the addition of sodium bicarbonate, and on this occasion the subject suffered a mild poisoning. Nevertheless, the chemical analysis showed that 100 and 104 mg of coca alkaloids were extracted and ingested in each experiment. The results obtained in this and in many other experiments demonstrate that alkaline substances potentiate the neurostimulant, cardiovascular, psychological, and metabolic actions of cocaine alkaloids.

Eduard Pöppig, the celebrated nineteenth-century German traveler in Peru, was the first to describe the symptoms and effects due to *immoderate* use of coca leaves. He discovered coca addiction several years before the discovery of cocaine. Many of the acute and chronic symptoms found by him among the coca addicts have been confirmed by recent studies. Among the more general symptoms due to a prolonged abuse of coca leaves, the most noteworthy are a pale or sallow complexion, a dry and grayish-yellow skin in the most peculiar cases, with more or less loss of subcutaneous fat and other signs of disturbed nutrition. Other common manifestations are muscular weakness (the coca chewers cannot work without the stimulatory action of the drug), apathy, introversion, and moral decay.

In general, coca addicts do not show, after an ordinary period of coca chewing, strong excitation symptoms, as cocaine addicts do. I have found, nevertheless, some physiological changes during the period of chewing, slight mydriasis, moderate increase of respiratory rate, rise of blood pressure, and definite increase of the heart rate. The spinal and autonomic reflexes are stimulated, and the cutaneous sensibility decreases. If larger doses are taken, disturbances of sensorial functions may appear. The metabolic alterations are very constant, even with small doses of coca; there is an increase of body temperature and remarkable increase in basal metabolic rate. The blood sugar always increases in experimental animals, but only in a very few cases does it increase in men after a period of coca chewing. In a few coca chewers we have found moderate depressive actions: a decrease of heart rate, blood pressure, respiratory rate, and body temperature. The coca chewers who ingest the largest doses of cocaine usually show stimulant reactions, and those who ingest the smallest doses may sometimes show depressive reactions. The period of chewing the coca wad lasts two or three hours; the stimulant reactions begin about half an hour after the chewing starts and are noticeable within three or four hours. The amount of alkaloids ingested per minute during the period of chewing varies between 0.3 and 1.6 mg, with 0.9 mg as the arithmetical mean.

If the addicted subject repeats the coca-leaf dosage, the stimulant reactions just described may last two or three additional hours, but if the chewing is stopped, a depressive state may follow. The heart rate and blood pressure show very low values, and a feeling of fatigue is experienced. The life of the addicted subjects is, consequently, an alternation of states of excitement and depression. Muscular strength of coca chewers during the abstinence period has lower values than muscular strength of nonaddicts. During the period of coca chewing, an increase of muscular strength was found in 50 per cent of the subjects studied. The action of coca upon the basal metabolic rate has been carefully studied, in addicts and nonaddicts. The results are the same in both groups. Even small doses of

Typical Andean market, with a woman selling coca leaves. The coca leaf is the single most important item of commerce in the Andes. (*Victor W. von Hagen.*)

leaves produce a great increase of basal metabolic rate; larger doses produce supernormal values. The effects of cocaine upon the basal metabolic rate have also been studied in addicts and nonaddicts. At a dose of 2 or 3 mg per kg of body weight, cocaine hydrochloride produces in most individuals studied a great increase of B.M.R. Supernormal values were obtained in some cases. Phenobarbital, given before the administration of coca leaves, decreases this action of the drug. Sodium bicarbonate or alkaline ashes (*tocra*), on the contrary, produce a great increase of the metabolic action of coca alkaloids.

Coca chewing affords a temporary relief of unpleasant sensations. It suppresses hunger, thirst, and fatigue. In the high altitudes of the Peruvian Andes, coca is taken by the natives to alleviate the sensation of cold, but in the tropical valleys it is used as a refreshing agent. It has been found that during the period of coca chewing there is a decrease of cutaneous sensibility, and this action is perhaps the explanation of the precedent contradiction. The acute psychological alterations produced by cocaism, at ordinary doses, are of minor importance. A general sensation of well-being and states of introversion, characterized by vividly real optative fantasies, were encountered in the majority of the cases. If the addicted subjects exceed their ordinary doses, which happens from time to time, some serious psychological disturbances may appear. The acute toxic symptoms of cocaism resemble those of acute cocaine poisoning. Alterations of perception, pseudo-hallucinations, delusions, obsessions, and other mental disturbances were found in some cases. Prolonged use of high doses of coca leaves may lead to states of insanity. At the ordinary doses, 30–60 gm per day, these incidents never occur. The mental symptoms disappear gradually after suppression of the drug.

No outstanding differences between the acute toxic reactions produced by coca were found among addicted subjects and those toxic reactions produced by equivalent doses of cocaine hydrochloride administered to nonaddicts orally. I must call attention to the fact that coca chewers take their ordinary doses of the drug during one or two hours, whereas nonaddicted subjects treated with equivalent doses of cocaine (2–4 mg per kg) given at one time, do not show stronger reactions. It must be noted also that nonaddicted subjects can chew 20–30 gm of coca leaves—the customary doses among addicts—without serious consequences. We may infer from these facts that coca chewers do not have an increased tolerance to coca alkaloids. Furthermore, I have shown that coca chewers treated with 3–4 mg per kg of cocaine hydrochloride manifest the same physiological and psychological reactions that are experienced during the period of coca chewing. The dictum of Tatum and Seevers, that "tolerance appears to be developed only to that class of drugs which produce a reduction in the activity of the cells" (which rules out cocaine), is confirmed among the coca chewers.

Abstinence symptoms appear only in the inveterate coca addicts. Even in these cases the symptoms were always slighter and less prolonged than those observed in other drug addictions. On the basis of this observation the apologists for coca chewing assert that coca is not a drug addiction. They forget, however, that in some cases cocaine addicts do not show abstinence symptoms, or only very slight ones. Moreover, they seem to forget that coca chewers show, as almost always among drug addicts, strong appetitive craving for the drug. Some years ago, Tatum and his collaborators showed that dogs treated with repeated doses of cocaine developed a strong desire for the drug, but they did not find evidence of abstinence phenomena. These discoveries have been very useful in understanding what happens in coca addicts. For several years I have made experimental studies of cocaine addiction. As a result of these studies I find that cocaine is among all the drugs that generate addiction—even including morphine—the one that causes the strongest cravings in experimental conditions. This reaction appears after a few days of the drug being given intravenously—more slowly if it is given orally. Abstinence symptoms are not demonstrable, however. It has been demonstrated that other stimulants of the central nervous system (caffeine, metrazol, coramine, and strychnine) do not create states of addiction in dogs, as cocaine does. These observations should put us on our guard with respect to the arguments of the apologists for coca, who claim that Andean natives need the stimulant action of coca leaves in the same way that others require the stimulating action of coffee.

Recently it was found that the great excitement shown by dogs before the expected injection of cocaine is accompanied by intense panting and tachycardia, and that these symptoms disappear as soon as the injection is given. The tachycardia may even be replaced by definite bradycardia. In nonaddicted dogs this phenomenon never occurs after the injection of cocaine. There is a powerful excitation, of both central and autonomic nervous systems, which can only be related to the craving state.

In some coca chewers, depressive reactions follow the administration of the drug, which may perhaps be explained as the result of a sedative action pro-

duced by the coca alkaloids, following the craving state. More important than the acute symptoms produced by coca chewing are the chronic symptoms, although one cannot attribute these complex chronic symptoms exclusively to the toxic action of coca alkaloids.

A study carried out upon Andean dwellers who are addicted to coca has shown a high frequency of degeneration stigmata and chronic pathologic processes. (It must be taken into consideration that the peoples of these regions have indulged in the drug for centuries.) The degenerative stigmata frequently found among the coca chewers are skeletal defects, craneal deformities, deaf-mutism, dwarfism, and facial asymmetries. Idiots and epileptics have also, according to recent observations, a very high frequency in regions of intensive cocaism. Among the natives of the Peruvian provinces where this drug is not used, or where it is little used, these defects are also found, but in very low percentages.

To find out whether the degenerative stigmata just mentioned are due only to the hereditary toxic actions of the coca alkaloids, or to these toxic actions associated with other pathogenic factors, is a subject for future research. In point of fact, malnutrition is always associated with coca chewing, and can be by itself, of course, one of the causes of racial decay and degenerative stigmata. But malnutrition is a cause of coca chewing as much as coca chewing is a cause of improper nourishment.

Upper-Amazonic tribes are also addicted to the chewing of coca leaf. Coreguaje Indians of the Caqueta River, Colombia, with bowl used for carrying coca powder. (*Richard Evans Schultes.*)

The most common pathologic defects found among coca-leaf addicts were hypovitaminosis, hyponutrition, ocular disturbances, enlarged thyroid glands and lymph nodes, and hepatomegaly. Of course, the toxic action of coca is not the direct cause of these disturbances, which can be the result also of the horrible living conditions created by the drug addiction and to other obvious factors. Hepatomegaly is perhaps the alteration most directly related to the chronic toxic action of coca leaves. The regular use of the drug over a long period of time, as Pöppig stated a century ago, leads to physical breakdown, occasionally accompanied by serious liver disturbances. In rats and dogs treated during several months or years with cocaine, we have found fatty degeneration of the hepatic cells, and other cellular lesions. The old inveterate coca chewers very often show a slight jaundice.

Glossitis and stomatitis are also very common symptoms among inveterate coca chewers. We have found glossitis in 50 per cent of several hundreds of examined addicts. The tongue presents a desquamation of the superficial epithelium, giving the appearance of the geographical tongue; sometimes it is dry, smooth, scarlet, and beefy-looking. These symptoms are probably due to the irritative action of coca leaves and the alkaline ashes used to increase the stimulant action.

The chronic action of cocaism upon mental activity deserves a special study. Johan von Tschudi, the nineteenth-century traveler, called attention to some mental features of the habitual coca chewers: "They can be recognized," says this author, "by their incoherent speech and their dull, apathetic attitude." Among the Peruvian natives there are remarkable psychological differences between those addicted to coca leaves and the nonaddicted ones. In the first group the introvert type predominates; in the second group, the extrovert prevails. In general, coca chewers present emotional dullness or apathy, indifference, lack of will power, and low capacity for attention. They are mistrustful, shy, unsociable, and indecisive. In advanced stages many of them are vagabonds.

It has recently been shown that a great percentage of illiteracy exists in the regions where coca is consumed in great quantities. A relation was found between the percentage of illiterate people

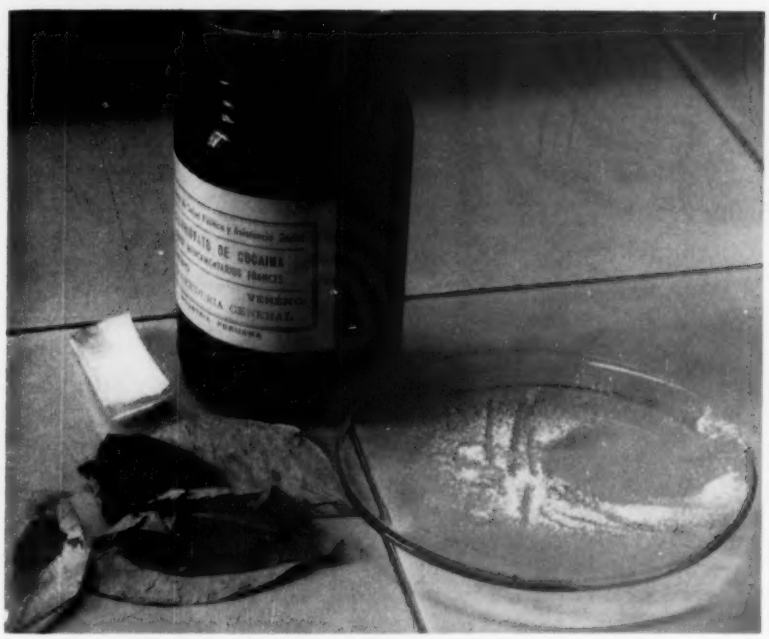
A typical house in the Peruvian Andes. There is a close relationship between poverty and coca addition. (*Victor W. von Hagen.*)

and the amount of coca leaves used annually—80–90 per cent in the regions of the largest consumption, 60–70 per cent in the regions of minor consumption, and 10–40 per cent in the regions where the drug is not used at all, or in very small amounts. In examining cases of children seven to ten years old, who had become coca addicts at an early age, I found that they became vagabonds and manifested very early symptoms of mental impairment; they had already acquired the characteristic pallid and dull-appearing face. Yet the mental disturbances disappeared after the withdrawal of the drug. School children between seven and twelve years of age often use coca. These young coca addicts are unable to learn, or learn with great difficulty. These observations led me to apply tests of intelligence to 500 coca chewers. The Binet-Simon, Porteus, and the Terman nonlanguage multimental tests were employed, and it was found that the mean IQ was 67 per cent. Only 19 (5 per cent) of the cases examined had normal values; that is to say, 80 per cent of coca addicts have subnormal IQ's. The lowest IQ's were found more frequently among coca addicts of long standing, whereas the highest IQ's were found more frequently among addicts of short standing. Research on the intelligence of nonaddicted Peruvian natives of the same social stock as the addicts was also carried out. The results of this examination of nonaddicts show higher IQ's than among coca addicts, with a larger percentage of normal cases.

The results obtained with attention tests showed that coca addicts have subnormal values. A relation was also demonstrated between the impairment

of attention capacity and inveteracy of coca habit. Nonaddicted native subjects have normal values in the attention test. Besides, the reaction time has a very high abnormal value among the long-standing coca chewers (360 milliseconds as an arithmetical mean) and lesser values among the short-standing ones (250 milliseconds as an arithmetical mean); the study of the personality of coca chewers with the Rorschach test has also revealed serious abnormalities. Notwithstanding the correlations found between the magnitude of mental backwardness and the length of the addiction, one cannot conclude that the drug is the only cause of the chronic mental disturbances. Some other factors must be taken into account (geographical and social isolation, economic misery, language barriers, deficient instruction, and underfeeding). Whether the coca alkaloids themselves produce a chronic toxic effect upon the brain that would lead to mental impairment is a question that cannot be answered at this time. The coca alkaloids are tropine derivatives, and some observations have been made which suggest that these compounds have some chronic toxic action upon the brain. Mexicans having the habit of smoking the leaves of a certain datura species called *toloachi* show the same apathy and dullness. Precisely the same phenomena occur in the use of *chamico,* a Peruvian datura, whose chronic use leads to dullness, introversion, and sometimes to a state similar to idiocy.

This research upon coca addiction is only a preliminary attempt to understand a subject that for some time has been highly confused by wide discrepancies of opinion concerning the action of the drug upon the health of coca addicts. Those who have written about the matter upon a strictly theoretical basis have arrived at the questionable conclusion that coca chewing is indispensable for South American natives living in high altitudes. They have even claimed that there is no absorption through the intestinal tract of cocaine extracted from coca leaves. Yet I have shown in other publications that coca alkaloids are not necessary for climatic adaptation. Unfortunately, the absurd position that coca is not harmful has always been a powerful argument to prevent all legislation aimed at the eradication of coca addiction.



The coca leaf and what it has wrought. Every community of over 25,000 population in the United States has its cocaine distribution center, supplied with cocaine from Peru. (*Photo by Victor W. von Hagen.*)

The Morality of the Machine

ELGIN WILLIAMS

Assistant professor of economics at Reed College, Elgin Williams has taught previously in New York University, the Universities of Washington and Texas, and the Columbia School for General Studies. During 1942–43 he was engaged in research work for what eventually became UNRRA. Author of The Animating Pursuits of Speculation *and articles in various scientific journals, he is especially interested in the unification of the sciences. He speaks here in a closely related cause: the movement for an ethical science and a scientific ethic.*

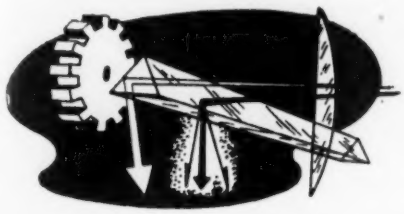
"WHAT will be singled out as the salient event of our time by future historians?" asks Professor Arnold J. Toynbee in a famous essay. "Not, I fancy, any of those sensational or tragic or catastrophic political and economic events which occupy the headlines of our newspapers and the foregrounds of our minds; not wars, revolutions, massacres, deportations, famines, gluts, slumps or booms, but something of which we are only half-conscious." What another historian-philosopher has called "the meeting of East and West" will seem in historical perspective, Professor Toynbee hazards, as the truly important happening of the first half of the twentieth century. For in the process of impact and counterimpact of the industrial Western and the agricultural non-Western societies whole civilizations—"the behavior, outlook, feelings and beliefs" of whole peoples—are being turned upside down.

I shall not be concerned with this larger civilizational process in the present discussion, although my subject—the impact of science and technology, of the "machine," on our American "outlook, feelings and beliefs"—is obviously related. The point of Professor Toynbee's to which I wish to call attention is his insistence that there are more important developments than show in the headlines. Specifically, I wish to urge the thesis that the most important product of science and technology at the present time is not penicillin or television or atomic energy or any of the other startling discoveries that usurp the headlines and occupy the "foregrounds of our minds," but a new way of thinking about the larger problems of the community in which these new gadgets and resources are appearing. In particular, science and technology, I believe, are affecting, although as yet only half-consciously, our way of thinking about the economic arrangements of "free private enterprise," or capitalism.

Changes in our way of thinking on economic affairs are recognized by all of us as overdue, and as absolutely necessary to prevent a depression which might create just the conditions in Europe which Stalin is counting on. As we remind ourselves that "prosperity is the best defense against communism," we remember the contrary melancholy truth that the Great Depression of the thirties was the Achilles heel of Western society that allowed Hitler to undertake the Nazi version of the world "revolution of destruction." Unless, therefore, our way of thinking derived from science and technology becomes conscious, and its adoption accelerated, we shall continue to fail to prevent the economic conditions that periodically give the Hitlers and the Stalins their opportunity.

Yet habits of thought associated with free private enterprise interfere with the change that all desire. Even as we recognize that our own "peculiar institution" shares responsibility for the spread of dictatorship and world war, we are uneasy in seeking alternatives and are therefore hampered in making a clean break with the past. In a real sense capitalist morality—even though in an unrenovated form it seems inseparate from depression and its series of calamitous social consequences—is still the only morality we have. This is the case because capitalism does provide a scheme for dealing with, after a fashion, the key economic questions which any economic system must answer. These questions are, What different things shall be produced? and, How much of each? Capitalism's far-heralded mechanism for dealing with these questions is known as "the allocation of resources by the price system," and the idea of this mechanism is so attractive to most of us that it stands in the way of the basic reforms that are necessary if Western

civilization is to avoid another war and possible extinction.

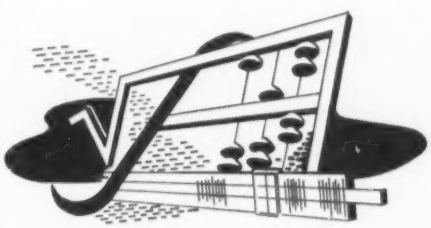


The idea of the price system is very simple. It is that of "consumer sovereignty." Under capitalism the decisions as to what things shall be produced and how much of each are made by the individual shopper. Every purchase in the "market" amounts to a vote of preference by the consumer involved, a directive to businessmen engaged in supplying goods and services. A great number of purchases recorded for any item causes its price to rise, and this is a signal for additional businessmen to drop whatever else they may be doing and produce the demanded goods. Their motive is profit, but the outcome is the production of just what the community wants. Thus self-interest is harnessed to the general welfare: resources are allocated in a process in which every individual participates, guiding by his purchases all the know-how of the community into the production of just those things the community wants and chooses.

The idea that the price system is "economic democracy" or the vehicle of "individual sovereignty" is very attractive—so much so that it is still extremely hard to give up—but for at least two reasons it gives an incomplete picture of the facts. In the first place, as everyone recognizes in time of inflation, the price system gives expression not to preferences but to buying power. In effect, it asks us not what we want but who we are. This is the case because of the extreme inequality of income under capitalism—which determines that, for instance, the wealthiest 10 per cent of the nation receive among them as much income per year as two thirds of the rest of our families put together. Obviously consumer ballots cast by such different voting publics differ widely—in this economic democracy a minority stuff the ballot boxes while most of the community are virtually disfranchised! In truth the price system does not allocate resources at all; this "system" is simply the arena in which we throw our economic weight around—if we have any. "Allocation of resources by the price system" is a euphemism for allocation of resources by the rich, who suffer with the rest of the community when the arrangements maintained by this euphemism collapse. In recent years the community has learned that not only does the price system fail to give expression to the desires of the community, instead serving as the errand boy of the wealthy in proportion to their wealth, but that periodically the system breaks down altogether. The extreme inequality of income which piles up massive incomes at the top of the heap leaves a virtual vacuum throughout wide portions of the community. Oversaving and underconsumption result; the rich fail to find investment outlets for their savings precisely because these huge savings at the top mean that the mass markets on which new investments depend are absent. The result is that progressive falling off of investment, production, and employment which spells depression. The industrial plant grinds to a stop.

Depression marks the greatest failure of the price system. That the rich get the greatest share of production is not particularly annoying, for many of them are active businessmen interested in and contributing in important ways to production; but that production should cease is unbearable. It is annoying, of course, that "allocation of resources by the price system" puts the conspicuous consumption and conspicuous waste of coupon-clipping playboys on a par with the machinery orders of active business managers. But more important is the fact that this "system" periodically fails to allocate resources at all, or, rather, that our land, labor, and capital goods in such periods of depression and unemployment are allocated to idleness and rust.

Apologists may perhaps say that "idle machines and idle men" are due to "failure of demand," implying that, since the price system is merely a medium for the expression of wants, the machines and the men are idle because nobody wants what they could produce. But many of us feel, on the contrary, that the idleness results from the failure of the free price system of free private enterprise to work as it is supposed to work, and for this reason the community is casting around for a substitute.



Nevertheless, the community has been unable thus far to come up with an alternative morality to do battle with the philosophy of consumer sovereignty. The most likely candidate was the philosophy of economic "planning." Those who put their faith in planning proposed a new answer to the questions, What shall be produced? and, How much of each? The community, they said, should sit down and take stock of what it wants and needs, utilizing not the mechanisms of the price system with its warped and self-defeating answers but the tested and reliable techniques of investigation, discussion, criticism, and experiment. Already, it was pointed out, this method is employed in science and technology and, indeed, in many other walks of life, even for such economic projects as the building of dams and highways; why not extend it to apply to the whole economic system?

Right at the outset this promising new departure in economic thinking ran into barriers which so far have proved insurmountable. First, there was the unfortunate fact that quite unsavory political regimes were loudly proclaiming to the world that they were planned societies at the very time that the opponents of capitalism were urging that the life of reason be extended to include production and distribution. Since the latter also spoke of planning, they quite quickly, and in great part undeservedly, were identified with the fascists and Stalinists and came in for all the obloquy which attached to them. Some of us are now wondering whether we have been too hasty in crediting Soviet Russia, for example, with being a planned economy, thereby taking the statements of its dictators at face value. Makeshift brutality, attempting to solve by club and whip problems that only democratic and scientific procedures of discussion and criticism are competent to deal with—this is what is going on in the Stalinist motherland, not "taking thought," which is, after all, what planning means. But, in the meantime, the revived propagandists for capitalism, overjoyed at this unexpected help at the count of nine, have managed to convince almost the whole community that any tampering with the sacred cows of free enterprise in the name of planning is synonymous with dictatorship. Capitalism may have its faults, but we had best reconcile ourselves to living with them, for if we attempt reform we shall succed only in establishing the same serfdom that the planned economy of Russia has brought.

Capitalism's counterrevolution would not have been so successful, however, if the doctrine of economic planning as it now stands did not have an inherent weakness. Certainly propaganda can convince a community that black is white, or that a David Lilienthal is a Hitler in disguise, but it can do so only in the absence of a stronger competitive idea. Despite the obvious defects of capitalist morality, with its reliance on the individualism of the free price system, the proponents of rationality in economic life have not yet come forward with an inspiring alternative morality. In a way, they are unsure of themselves, and even tacitly or openly admit that planning might perhaps involve a little coercion, or at least a centralization of decision-making in the hands of a small group of planners, thus giving the whole game away to their capitalist detractors.

Although this diffidence is unnecessary, and although *real* economic planning does not involve centralization of authority or coercion of individuals, the confusion and backtracking of the opponents of capitalism are understandable. For, although they are no longer capitalists, they are still individualists, and the peculiar kind of individualism that characterizes the Western world is definitely inimical to thinking in terms of an alternative morality to that of the price system, even when one is trying with might and main to do so. For this individualism assumes that what should be done in the community—morality, in other words—cannot be a matter of voluntary agreement; individuals either make their own unique subjective choices, or they are coerced by other individuals. As individualists, therefore, even the economic planners cannot conceive of settling the problems of production and the allocation of resources except by reference to the wants and desires of individuals—and this lands them right back in the toils of the price system that gives expression to these presumed individual cravings. The planners have not indicated any *objective* criteria for the allocation of resources, and until they do planning will be opposed by the community on the ground that it gives a few men a great deal of arbitrary power. Actually, just the opposite is the case. Not long ago an official of a national small businessmen's organization called the heads of America's leading corporations a "Politburo," and that is the reality of minority control which the planners would have us escape. But at the present time the community is not convinced that planning can be objective—can, in other words, represent anything but the ideas of the planners—and until it is so convinced it will continue to entrust its economic destinies to the price system, which under the guise of consumer sovereignty

actually works out to give a few men just the arbitrary power that is feared.

Is there an escape from this dilemma? Is there an objective alternative to capitalist morality? There certainly is, and it has been lurking in the statements of the economic planners all the time, even though they themselves have been too abashed in the face of torrents of criticism and mudslinging to recognize and use it. The answer to the questions, How should we use resources? and, How should we decide how much of what things to produce? is their old answer. We should decide scientifically by social discussion and agreement just as we decide so many other questions already, despite our mistaken individualism. The dictates of industrial civilization, not those of all-powerful central committees, nor yet those of all-powerful corporate magnates, should be our guide. Doing and making what the turbines and railroads and factories tell us: this is the objective morality of the machine, and this is what it tells us: make more turbines, factories, railroads; allocate resources to make more machines to allocate more resources.

These statements, unfortunately, tend to meet only with derision. Everyone is familiar with the answer to people who say such things, or who are even suspected of saying such things. Such worshipers of the machine-god are elevating a mere means to the status of an end-in-itself, trying to take all the value and dignity out of human life and reduce it to the synchronized and deadening monotony that Aldous Huxley describes in his *Brave New World*. Or at the very least such proponents of the morality of the machine are dodging the question. The question is not, Shall we make machines? but, What shall we use them *for?* To what *ends* shall they be turned? About these questions the machine and its handmaiden science can tell us nothing. They are very helpful in making things for us but they tell us nothing at all about the uses to which these gadgets shall be put, the ends they should subserve. If these questions are not settled first, if we go off the deep end thinking that there is something inherently valuable about machines as such, we shall find ourselves making machines to make more machines to make more machines, flooding the world with dime-store merchandise, using $10,000,000 cyclotrons to produce a better grade of popcorn.

If we are ever going to escape the tragedies of capitalist morality—and time is growing short—we should recognize at once and without qualification that such allegations are nonsense. It simply is not true that machines do not tell us anything about the uses to which they should be put. So far from this being the case, just the opposite is true. *The use to which a machine should be put is determined by the machine itself; it is inherent in the machine itself; it is inherent in the very idea of a machine.*

On the simplest level this is obvious to those most worried about the erection of machines into absolutes and therefore about the problem of determining the ends which these mere means should subserve. In every particular case both the proponents and the confused opponents of capitalist morality recognize that science and technology tell us what should be done as well as what is. After all, none of them ever asks: What shall we use the new mechanical cottonpickers for, picking cotton or digging potatoes? And what about jeeps—should we milk them or ride them? Even those most rabidly derisive of the idea that machines themselves tell us to what uses they should be put never shave with their fountain pens or carve their Christmas turkeys with can openers.

On a more recondite plane they usually recognize that even when tools are used in ways they were not originally intended to be used—as in invention or artistic innovation—there is some inherent qualification for the new job. Edison used tungsten for a new purpose, but he used tungsten, not catsup. Finally, when it comes to still more general issues, there is some (perhaps unconscious) inkling that if, for instance, atomic energy is used for military purposes it will destroy even its users; *therefore,* it is inherently not intended to be so used and *is* intended for beating swords into ploughshares and suchlike activity.

Yet those concerned to erect a new morality still do not see that such cases point to technology as the locus of value. They still put their questions in the old way. They still deny any obvious connection between machines and their uses; they still refuse to generalize from all the particulars and conclude that our guide in economic affairs should be the internal dynamic of industrialism itself. Science's inherent morality goes unobserved and, indeed, is denied out of hand.

Yet it is exactly inherent morality which science and its copartner, industrial production, have to offer. Our search for real values would be greatly facilitated if we would realize that the uses, ends, and concomitants which machines should have are not arbitrary but are determined by the machines themselves. Our very resources will tell us, if we listen, how they should be allocated.

Take any number of examples. The very existence of dynamos is conditional upon their being used to run the mills where the steel is forged to make dynamos. Similarly, if we are to have railroads, they must be used to carry the steel to make the dynamos which make the electricity to run the railroads. Every use of the machines can be stated in such a circular sentence to bring out the internal requirements and interrelated conditions requisite to carrying on the industrial order, and it is these interrelated conditions and requirements, laid down not by arbtrarily constituted authorities nor yet by arbitrary individual wants, which make up "the morality of the machine." These requirements are both qualitative and quantitative. What shall be produced? Tires, spark plugs, windshield wipers, bumpers. How many of each? The automobile tells us—multiply the number of automobiles by five to get the number of tires, by ten to get the number of spark plugs.

Obviously to speak in this way of the morality of the machine is in a way poetic license. Some human regime must determine what kinds of machines and how many are to be manufactured and put into operation. There is, nevertheless, a good reason for speaking metaphorically in the face of this fact, and for this reason the community, I believe, would make a great mistake in allowing apologists for free enterprise who continually dwell on the fallibility of planners to prevent the economic change so urgently required at the present time. The point is this: scientists and engineers are human too, yet they are guided in their activities by objective relationships existing outside of them, and in a very real sense can be said to be "cooperating with Nature." There is no reason why we should not in similar fashion cooperate with the machine, or be guided in our economic procedures by industrial civilization's objective conditions of existence, and this is all that is urged here.

True, our machines would not dictate directly; their wants and desires would have to be interpreted, just as are nature's. But if the machine were accepted as the truly valuable part of society, if we decided at every crossroads to facilitate *it* instead of the free price system, disagreements about interpretation could be resolved in economics in the same way they are at present resolved in physics and chemistry: by recourse to the facts. For what is good for the machines is open to objective determination. There is no question that science and engineering can tell us what to do to keep our machines in good health, provided we agree to let them. The question the community has to answer is, Shall we be guided in our economic decisions, in our allocation of resources, by science and engineering, or shall we continue to be guided —or rather, misguided—by the wants and desires of consumers expressed in the free price system?

Misled by propagandists for the *status quo* and by its own inherited habits of thought, the community may reject the morality of science and engineering, to its material loss and political peril. Among other things, it may understand by "science and engineering" *scientists* and *engineers,* and reflect that these folk are human, fallible just as businessmen are, and therefore refuse to jump from the frying pan to what might turn out to be the fire. But, if we cling to our present ways under these assumptions, we shall have forgotten that in a very real sense the scientific method has eliminated man's ancient fallibility and individualism, or at least made them susceptible to discovery and control. In changing science and engineering for businessmen, we should not be merely changing one set of *people* for another, but substituting an entirely different *method* and procedure for our present "system" of handling economic problems. This method is truly one where autocracy and dictatorship have no place: it does not matter who you are in the world of science if your experiments do not check out. Or, if one man makes a mistake, another will catch it almost automatically. For a society based on science to be caricatured as authoritarian is ridiculous, because science and technology are the prototype of "government of laws and not of men."

Charles F. Kettering, the grand old man of the General Motors Corporation, was invoking the morality of the machine, I think, in his reply some years ago to a question concerning his authority

in the research laboratory. Though they call me "Boss" Kettering, he said, I'm not the boss: nobody is. When we are trying to improve a motor and want to know what to do, "We set up the motor and let *it* talk." It is in this sense, which is really meaningful and not just poetic metaphor, that we can speak of the dictates of the machine, of what our motors and engines as a whole are trying to tell us. When we speak in this fashion we are merely indicating that more than our private opinions are involved, that anyone can check orders and decisions based on mechanical requirements and come to the same conclusions himself.

The dictates of the machine, the internal mechanical requirements of industrialism, go far beyond merely physical decisions in the making of automobiles, railroads, and dynamos. Already it is becoming clear that social arrangements are also involved. Interestingly enough, the focus here has been at the very point at which capitalist morality broke down so disastrously: the distribution of income. It is now a commonplace that "mass production is impossible without mass markets," that full production calls for full consumption, and so on, although we have not yet taken the steps to reduce income inequality which this perception requires.

Other, related perceptions are also becoming commonplace, and when theoretically generalized will be recognized as "scientific morality," or the ethics of the machine. Thus we perceive that industrial civilization must be used to house and to employ and to educate our *whole* people if they are in turn to be able to operate and maintain industrial civilization. If we are to have machines at all, in other words, we must necessarily also have peace, tolerance, and abundance; i.e., we must use machines peacefully, without discrimination, and to the hilt. These are parts of the machines as surely as are nuts and axles, and the blueprint of a factory calls for high wages and increasing consumption as surely as it calls for assembly lines.

Similarly, to have machines we must have science —and science cannot live, much less flourish, without freedom of inquiry, liberty of expression, opportunity for education and criticism, respect for the dignity of the individual. The idea of having machines *includes* the idea of having peace, full and creative employment, pleasant jobs and cities, the four freedoms. Just as definitely the idea of having machines *excludes* slums and malnutrition, which cripple workmen; prejudice and war, which interrupt work and condemn men to rusting idleness or corrupting violence.

Here I must pause to answer an objection. Surely, someone will say, you are forgetting Hitler? To him the idea of having machines certainly did not include all the democratic values you mention. He certainly used machines for destructive purposes, and so have countless other people. If machinery and science are so democratic, how do you explain their existence in the totalitarian countries? I should answer these questions in this way: The totalitarian countries have limited their scientific advance and mechanical might to just the extent that they have limited human freedom. If Hitler, for instance, had not locked up or killed off an important portion of Germany's scientists and intellectuals, her industrial progress could have been incomparably greater than it was. We sometimes speak of fascism as "efficient." What could be more foolishly inefficient, from the Nazis' own point of view, than exiling such atomic scientists as Lise Meitner on the ridiculous grounds that they were non-Aryan?

Men and societies can *try* to turn machines to inherently illogical (and immoral) purposes, but such acts are self-defeating. This is true not only for dictators, who discover that to use machines for war purposes is to raise up their own executioners. In just the same way capitalists will destroy capitalism, I think, if in the next few years they use machines to "maintain profits;" i.e., if they allow our technology to lie idle half the time. Finally, if politicians continue trying to disregard the morality of the machine and consequently use machines to preserve national sovereignty, they are going to blow up the world, they are going to blow up national sovereignty, and they are going to put an end to politicians too.

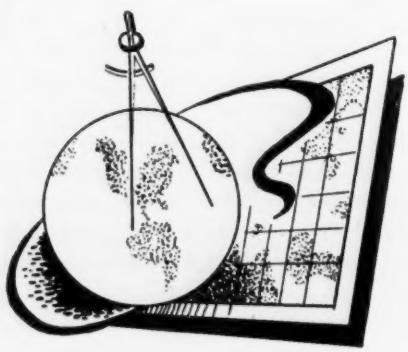
If, on the other hand, the world comes to accept the industrial ethic, it will have a principle for the allocation of resources which will hasten the disappearance of the threats to the individual and his freedom which now make life a terror. Not only this, but mankind will be on the way to a life in which those finest achievements of the human spirit, art and culture, will become the ordinary, accustomed, and uninterrupted dedication of the community. Far from leaving out of account the nobler sides of human nature, as critics of the new morality sometimes urge, the principle of "making more machines to make more machines to make more machines" is the artistic, creative principle itself, generalized and erected into a guide. For what is art itself? Can it be characterized in any other way than making sonnets to make more sonnets to make more sonnets, painting pictures to

paint still other pictures? Nor are these "creations" ends-in-themselves. Just as the meaning of railroads is vacations and travel which "broaden and enrich the personality," so having paintings means enjoying Sundays in art galleries, "capturing moments of eternity," "halting the fleeting flow of time for the instant," and all the rest. Indeed culture is not a special case but one with industrialism; there is an interdependence between the fine and mechanical arts, in which (for instance) painting's gift of linseed oil to the printing industry has been handsomely recompensed by modern technology's addition of a host of new materials to the artist's repertoire. It is absolutely impossible, moreover, to separate industry and culture in any given case. Is architecture art or construction? Is Boulder Dam awe-inspiring or efficient? Is a Stradivarius a masterpiece of craftsmanship or a work of beauty?

No, making machines to make more machines does not exclude making sonnets, but includes such activities by definition. That is why the distribution of resources under the industrial ethic will allocate books and leisure and opportunities for work and study to artists, instead of the grinding poverty and wasted talents too often meted out to them under the morality of the free price system of capitalism. For, even these days, with many vestiges of capitalist ethics still clinging, the community recognizes that artistic activities are liberative of the whole of life; the artist is among other things a critic who enables us, as we frequently say, "to see ourselves for the first time." We run our machines better for a Dickens who shows that poverty does not conduce to industrial progress and for a Kafka who warns us when the machines are turning into a bureaucratic Apparatus; these and others like them are our real efficiency engineers. With the self-conscious adoption of the ethic of industry, the morality of science, we shall not only be on the way to ridding the world of the curse of depression, war, and totalitarian dictatorship but toward creating a society in which art and culture can flourish as never before. Prerequisite to this moral revolution is an intellectual one. It is up to us whether we shall decide in time that the capitalist morality of allocation of resources by the price system is a fraud and that the pooh-poohing of allocating resources by the inner logic of industrialism as nonsense is itself nonsense.

Our business and political leaders are for the most part captives of price morality in their social thinking. So are our engineers and scientists. But within the factory, in the laboratory, another principle for the allocation of resources is followed. The scientists and engineers use it, and the businessmen and politicians tolerate it, to a degree. Both groups must now lead the way to a generalization and a freeing of this method, to the adoption of the morality of the machine in the larger community.

This wider adoption may turn out after all to be the "cultural synthesis" which Professor Toynbee decries. Science and technology have already made the world one physically. Because they are the locus of freedom, the prototype of the classless society, the antithesis of arbitrary dictatorship, they now have a chance—or, rather, taking our hint from the machine and science, *we* and, above all, our leaders now have a chance—to make the world one morally.



The Role of Boron in the Agricultural Regions of the Pacific Northwest

C. G. WOODBRIDGE

Dr. Woodbridge received his formal education in British Columbia and in the state of Washington (Ph.D., State College, Washington, 1948). His main field of investigation has been the study of certain physiological disorders, especially in pomaceous and stone fruits, caused by a deficiency of some one of the micronutrients. His article is based on a paper presented in a symposium on "Minor Element Nutrition in Western Soils" at the June 1949 meetings of the Pacific Division of the AAAS in Vancouver.

AGRICULTURAL production has increased markedly in the past few decades as scientists and farmers have intensified their efforts to produce more from a given area. Much of this increase can be attributed to a greater knowledge of the efficacy of inorganic nutrition. The need for the plant nutrients—nitrogen, phosphorus, potassium, calcium, magnesium, sulphur, and iron—has long been known and recognized. However, the real value of the micronutrients—boron, manganese, zinc, molybdenum, and copper—has become better appreciated since they have been shown to be essential for plant growth, even though required in extremely small amounts. For example, for many plants a boron concentration in the growing medium of one half of one part per million is quite adequate. Of the micronutrients, boron is now of great economic importance in the Pacific Northwest; that is, in the states of Montana, Idaho, Washington, and Oregon and in the Canadian province of British Columbia.

Boron is an element, just as is nitrogen, potassium, or phosphorus. It does not occur in the free state but is found in combination with other elements, mainly oxygen, sodium, and calcium, in such minerals as borax and colemanite and in complex silicates and aluminates such as tourmaline. Of the elements which make up the earth's surface, it ranks forty-eighth in abundance, yet this is only about 0.001 per cent of the earth's crust. Fortunately, some of the deposits of boron are highly concentrated, and boron compounds, especially boric acid and borax, are readily and cheaply prepared. On this continent, notably in the Death Valley region of California, boron compounds are abundant. In other parts of California, in most other states, and in many provinces of Canada, boron-deficiency symptoms on plants have been recorded. Evidence indicates that there are minute traces in most soils, but frequently these amounts are insufficient for the normal growth of agricultural crops. It has been suggested by Eaton and Wilcox, of California, that if an area is not fundamentally low in this element, deficiencies may be caused by anion exchange, molecular absorption, or chemical precipitation. Analyses of sands, rocks, and irrigation waters show that many areas of the Pacific Northwest are naturally low in boron.

The presence of boron in plants was first shown by Wittstein and Aporger in 1857, and in 1910 Agullhon and Bertrandt, working in France, reported that some plants grew much better when boron was available in the growing medium. It remained for Warrington in 1923, however, to show beyond doubt that boron was essential for the growth of the broad bean, *Vicia fabia,* and it is now accepted that boron is essential for the growth of all plants. In the Pacific Northwest, Dr. H. R. McLarty and associates (Fig. 1) of the Dominion Field Laboratory of Plant Pathology, Summerland, British Columbia, first showed that this region was deficient in boron.

The symptoms of boron deficiency in plants as described by various workers vary with different crops and different conditions for growth. On some plants, a general weakness is characteristic; on others, distinctive symptoms may be found; and on still others, there may be no outward symptoms but an

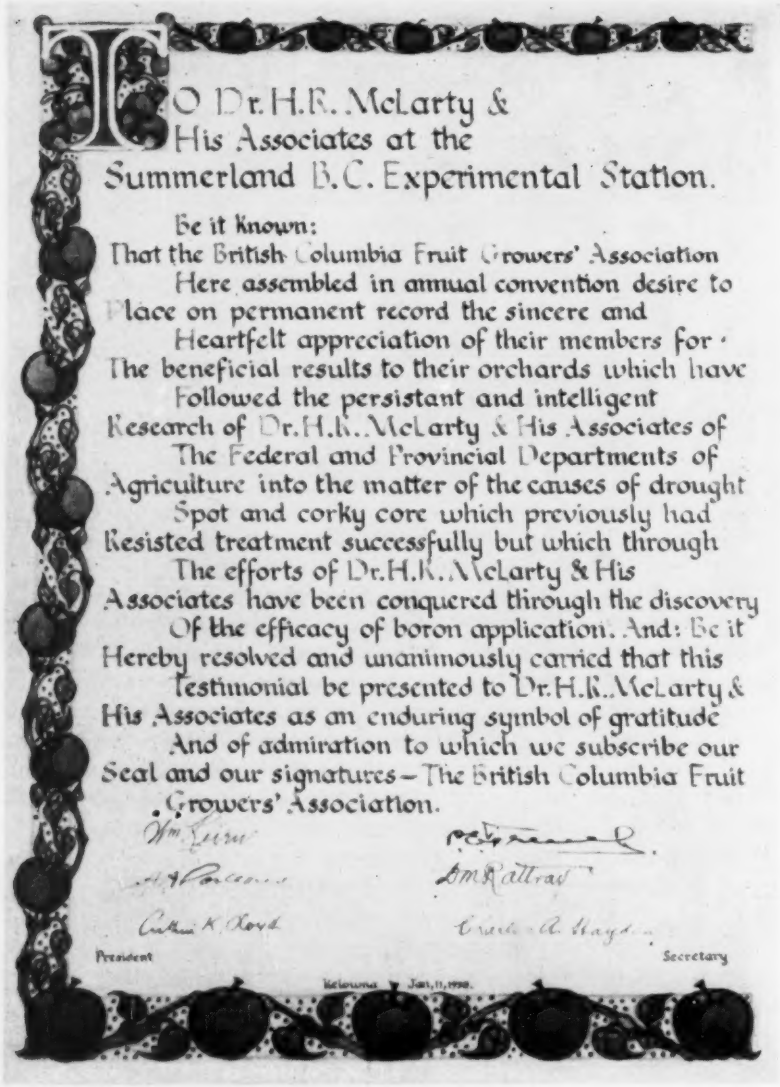

To Dr. H.R. McLarty &
His Associates at the
Summerland B.C. Experimental Station.
Be it known:
That the British Columbia Fruit Growers' Association
Here assembled in annual convention desire to
Place on permanent record the sincere and
Heartfelt appreciation of their members for
The beneficial results to their orchards which have
Followed the persistant and intelligent
Research of Dr. H.R. McLarty & His Associates of
The Federal and Provincial Departments of
Agriculture into the matter of the causes of drought
Spot and corky core which previously had
Resisted treatment successfully but which through
The efforts of Dr. H.R. McLarty & His
Associates have been conquered through the discovery
Of the efficacy of boron application. And: Be it
Hereby resolved and unanimously carried that this
Testimonial be presented to Dr. H.R. McLarty &
His Associates as an enduring symbol of gratitude
And of admiration to which we subscribe our
Seal and our signatures – The British Columbia Fruit
Growers' Association.
President
Secretary

FIG. 1. Illustrated address presented to Dr. H. R. McLarty in appreciation of his finding a cure for drought spot and corky core.

effect on productivity such as decreased yield and viability of seed crops. Depending upon the boron requirement of the plant being grown, the timely application of borax or boric acid at rates varying from 10 to 50 pounds per acre usually gives effective control.

Pomaceous Fruits

By 1920, in widely separated parts of the world —Australia, Finland, the United States, and Canada—certain physiological disorders on apple had been recognized and had been given descriptive names such as "drought spot," "corky core," and "dieback." Drought spot (Fig. 2) is a name given to a condition of the fruit in which the epidermis and underlayers of cells have turned brown, become corky, and died. The calyx end is most commonly affected, but deep cracks may extend over the surface of the fruit. This injury often appears when the fruit is only one-half to one inch in diameter, and the malformation increases as the fruit develops. Corky core, cork, or internal cork (Fig. 3) is chiefly confined to apples; it is characterized by a condition within the fruit, and no external symptoms are in evidence. Yellowish-brown, corky areas appear in the flesh of the fruit, in and around the core area. On some varieties symptoms of boron deficiency are quite different. In Delicious (Fig. 3), the fruit becomes flattened, and the five points at the calyx end, so characteristic of this variety, are no longer prominent. In the Snow (Fig. 3), internal cork may be characterized by a series of depressions and protrusions in the surface of the fruit. Dieback (Fig. 4), which may affect most fruit trees, refers to a condition that

FIG. 2. *Left:* Healthy McIntosh apples. *Right:* Drought spot.

develops on the twigs in the spring. The twig or limb appears quite normal during the dormant period, but as the spring season advances the buds either fail to develop at all, or else put out poor, unhealthy growth which later shrivels and dies, even though the cambium is alive at this time. An application of a boron compound as a spray in the spring has always restored the vitality of the affected parts.

In the Western states these disorders have been noted wherever the apple is grown commercially, and many workers have corroborated the above

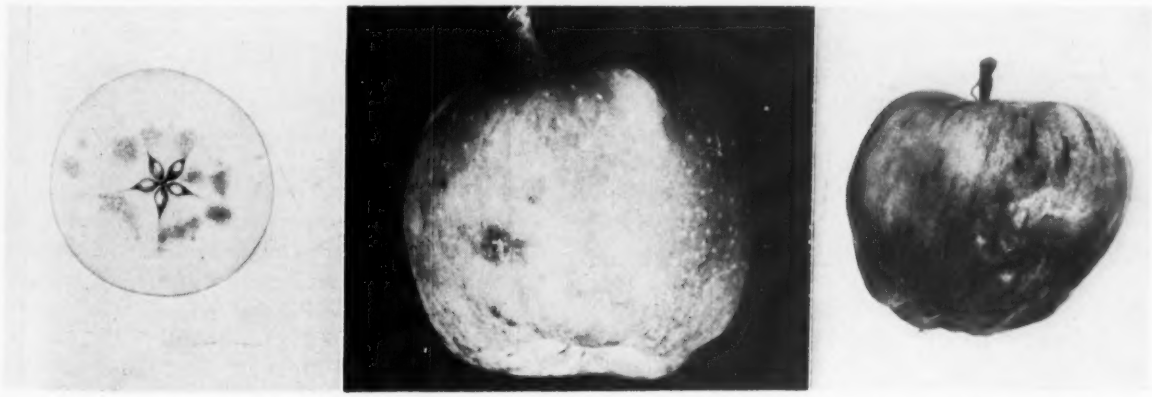


FIG. 3. *Left*: Corky core in McIntosh. *Center*: Corky core in Delicious. Note the absence of the five points so characteristic of this variety. *Right*: Corky core in the Snow variety is characterized by a series of depressions and protrusions.

FIG. 4. Dieback on apple at blossomtime.

findings. It has been stated that when the diseases are severe the boron content of the twigs is below 10 ppm on a dry weight basis, whereas in twigs from healthy trees the boron content does not usually fall below 14 ppm. In fruit the contrast is much more marked—diseased fruit often contains as little as 2 or 3 ppm boron on a dry weight basis, whereas healthy fruit contains up to 20 ppm.

In British Columbia the economic losses caused by the presence of these disorders had become so great by 1930 that a special Physiological Disorders Committee, consisting of members from the British Columbia and Canadian Departments of Agriculture, was formed. By 1933, McLarty had reasoned that these physiological disorders were most probably caused by some mineral deficiency or mineral unbalance within the trees. Following up this line of reasoning, he injected severely affected apple trees with some thirty different chemicals. In these experiments, the dry test material was packed in holes drilled into the trunks of the trees. The holes were about one-half inch in diameter and two inches deep. After having been filled, the holes were sealed with a commercial grafting compound. The dry materials were used because of the convenience of handling and also because greater amounts could be used without injury to the foliage. The following year the crop of two of the injected trees was practically free of the disorders, and it was noted that one of these trees had been injected with boric acid and the other with manganese borate. The trees injected with manganese compounds, other than the borate, showed no change. Following up this lead, forty trees were injected with either boric acid or borax in the fall of 1934. In the summer of 1935 every tree that had been injected the previous fall showed none of the diseases or a very low incidence of them. Because of the great economic losses which many of the growers were suffering that year, the committee decided that it was well worth while to make an immediate recommendation that all affected trees be injected with boric acid crystals. Further experiments showed that under the conditions prevailing in the Okanagan Valley, effective control could be obtained when either boric acid or borax was applied by broadcasting it onto the soil by a method similar to that used with fertilizers or by

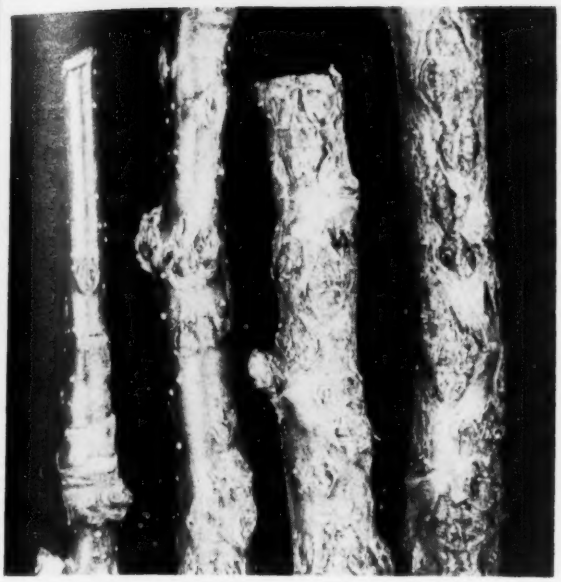


FIG. 5. Bark canker on apple twigs.

dissolving the compound in water to make a 1 per cent solution and then spraying the experimental tree. Chemical analyses were made on tissues from affected and nonaffected trees, and a positive correlation was found to exist between the incidence of these diseases and a low content of boron.

A general recommendation in the Pacific Northwest is that all orchard land where boron deficiency disorders have occurred be treated every third year with boric acid at the rate of 30 pounds per acre, which amounts to approximately one-half pound per tree in a well-established apple orchard. On some experimental plots single applications have prevented the reappearance of these disorders for seven years, but in others, especially where the soils are light and sandy, a single application has been effective for only two to four years.

A word of caution about the use of boron compounds had to be passed on to farmers within a few years of the initial recommendation because many wished to apply the chemical more frequently than every three years. Wilcox and Woodbridge in Canada, and Haller and Batjer in the United States, studied the effect of boron on the storage qualities of apples. Both groups of investigators found that heavy soil applications of boron compounds caused a characteristic browning of the flesh of the McIntosh apple fruit and increased the amount of water core and flesh breakdown in Jonathan fruit. In cases of severe breakdown the boron content of fruit was over 100 ppm—about four to five times that found in healthy fruit.

The treatment with boron has, since its initial recommendation, added millions of dollars to the fruitgrowers' income. An accurate evaluation of the increase in marketable fruit is difficult, but examination of records in British Columbia suggests that this annual increase is between 500,000 and 1,000,000 boxes (40-lb.) of apples alone.

Boron-deficiency symptoms of pear are somewhat similar to those of apple, but in the field the diagnosis is complicated by the presence of other unrelated troubles and abnormalities—that is, black end or hard end, russet, and, on the Bosc, a virus, stony pit. On the Bosc and Bartlett varieties, a low content of boron is characterized by shallow depressions, more abundant near the calyx end, in which the skin is a darker green than on the surrounding areas. The flesh immediately under the depressions is corky in nature. Cracking is common, especially on Bosc, and the extent of this abnormality varies from short shallow breaks in the epidermal layers to deep sutures that completely circle the fruit and usually cause much malformation. An excessive amount of cell formation near the calyx end is due to a low level of boron. Symptoms of boron deficiency on both pear and apple also appear on the twigs of severely affected trees. They are manifest in irregular and superficial bark cankers on younger branches (Fig. 5). There are fewer leaves on diseased twigs, and sometimes only one or two sets of dwarfed terminal leaves are present. In most severe cases a dieback of the twigs occurs in the spring, and the tree dies over a period of a few years.

It has been shown that some types of fruit cracking, some types of rough bark, some kinds of black leaf, and black end of fruit are not due to a boron deficiency, although before the boron-deficiency symptoms were defined they were considered as being related to drought spot. In Table 1 a brief résumé of some results of analyses is presented.

TABLE 1

THE BORON CONTENT OF PEAR TISSUES

Tissue	Condition of Fruit	Boron ppm Dry Weight
Twigs	Severe drought spot	7.3
"	No drought spot	16.3
Leaves	Healthy	37.7
"	Slight black leaf	35.3
Fruit	Speckled and cracked	60.4
"	Healthy	49.3

Stone Fruits

In British Columbia boron-deficiency symptoms

have not manifested themselves in fruit of apricot, but twigs show the characteristic dieback condition. Foliage symptoms of boron deficiency are the same both in sand culture and in the field, and can be identified by thickened midribs, laminal distortion, and a characteristic boatlike form (Fig. 6). In the state of Washington boron-deficiency symptoms of apricot fruit have been described by Bullock and Benson as internal browning and corky tissue around the stone cavity. In severe cases irregular cracks may develop on the surface of the fruit, and considerable malformation may also be caused by a shriveling of the epidermis.

In peach, the sudden dying back of limbs and sometimes of the whole tree in the spring, which in the previous year had appeared quite healthy, is a symptom of boron deficiency which is met in British Columbia. The first signs of this trouble can be noticed only as the buds begin to swell. Those of affected trees size at a much slower rate than those on reputedly healthy trees, and their growth soon ceases. As with apple, the tissues are still alive, but they do not have the power to grow. In some experimental plots and in some grower-owned orchards young peach trees have been injured by the excessive application of boric acid. Yellowish-green patches and brown-to-black markings on the midvein on the underside of the leaf are characteristic, and sometimes small patches of tissue bordering the midvein will become brown, die, and fall out. When affected leaves were analyzed, the boron content was 124–138 ppm, whereas that of leaves from healthy trees was 40–50 ppm on a dry weight basis.

Haas and Miller, Schuster, and Stephenson have described boron-deficiency symptoms on walnut trees growing in California and Oregon, respectively. The mature leaves of seedlings when grown in boron-deficient cultures were curled and brittle, with large, irregular, dark-brown areas between the veins. The leafstalk and the midribs of the leaf were sometimes split. When the boron deficiency was extreme, the meristematic tissues at the tip of the shoot died and a progressive backward death of the twigs took place. In the field the symptoms were somewhat different. They have been described as a leaf-scorch condition and dieback of the twigs. In cases of mild deficiency a cluster of leaves may be produced at the tip of a shoot, and here dieback does not usually occur. The leaves are a lighter green and cupped in shape. In boron-deficient trees the nuts do not set properly, and many drop when they are about the size of large peas. In the leaves, boron toxicity is characterized by marginal necrosis, which develops without a noticeable yellowing and may extend back to the midrib.

Vegetables

Boron-deficiency symptoms on vegetables vary greatly from crop to crop. In general, the meristematic tissues are the most severely affected. This

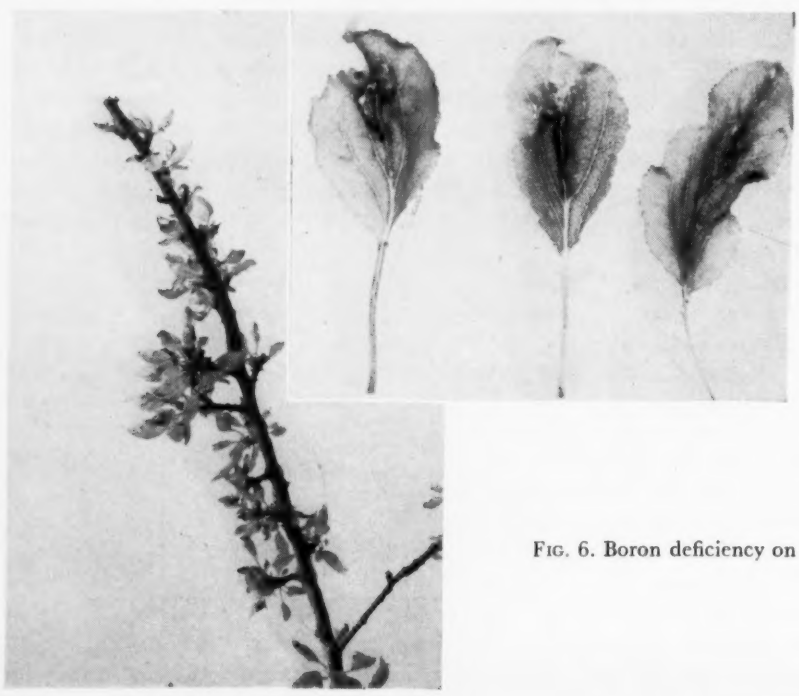


FIG. 6. Boron deficiency on apricot in twig and leaves.

implies that the element boron, in some combination, is necessary when cells are dividing and when new cell walls are being formed. It has been suggested that boron plays a part in the structure of the middle lamella, and this is substantiated by the cell breakdown that occurs when root rot takes place in some vegetables, such as beet and turnip.

In table beet a disorder due to boron deficiency is commonly called black spot. Small black spots may appear on or in the flesh of the root, usually during the latter part of the growing season. If they arise near the surface of the root, they frequently develop into cankers. It is believed that certain soilborne organisms may enter the beet through these cankers and adversely affect the soundness of the root in the field, and especially during storage. In severe cases of boron deficiency, death of young crown leaves occurs, and the plant center becomes open, as with most root crops. In an experiment in British Columbia, beets were grown with and without boron, and at harvest the crops were stored separately over winter in shallow pits. On examination the following spring those that had been supplied with boron were in excellent condition, and only a few showed any evidence of decay. The roots from plots not supplied with boron, however, were of poor appearance, and 40 per cent showed decay. Some of these low-boron roots were planted the second season, and, although they all showed new crown growth at the time, it was weak and about one third of the plants died. Those that lived were stunted and set only a small amount of viable seed, yet a comparison of the germination of seed from roots grown with and without boron indicated no significant difference.

In mild cases of boron deficiency in carrots, there are frequently no external symptoms, but the plants have a weak appearance and are somewhat smaller than plants having an adequate supply of boron. In the roots large cavities may develop and black necrotic areas may be present. Sometimes there is only a darkening of the flesh, and the core is watery. Roots deficient in boron have poor keeping qualities and are more subject to decays. It has been reported that very high and very low nitrogen aggravates root decay even in the presence of boron. The foliage may show signs of marked boron deficiency. In this connection Woolliams states: "There was a tendency to form strap-like leaves with all degrees of modification from slight reduction in segmentation of the leaves to extreme cases where the whole leaf was reduced to a single grass-like blade."

"Brown heart" of turnips caused extensive losses in most areas where this vegetable was grown until it was demonstrated that it could be prevented by the application of boron compounds. In the field, symptoms of boron deficiency can be seen only on cutting the root. In artificial culture, and whether grown in sand or soil, a healthy root has a smooth skin, whereas a diseased one has a rough and cracked surface. If the boron deficiency is severe, one of the earliest and most common symptoms is the death of the tissues in the growing tips. With healthy seed, plants not supplied with boron appear to grow normally for six to eight weeks; then the leaves become yellowish, mottled, distorted, and eventually die without the formation of a fleshy root. In the root the disease appears as brownish or water-soaked areas. These symptoms are repeated at irregular intervals in the parenchyma tissues and do not extend outward as far as the cambium. In storage, the areas of diseased tissues become grey and punky but do not change in number or extent. There is some evidence to indicate that this disorder is more prevalent during dry than during wet seasons, and it is believed that heavy and continuous rains may facilitate the availability of boron to plants.

When celery is grown on a medium containing insufficient boron, longitudinal brown stripes are often present on the stalks, and crosswise cracks may develop. The tissue adjacent to the crack may curl back, and the leaves sometimes have a mottled brown appearance. The plant is usually stunted, the petioles are brittle, and the young leaves may shrivel and leave a gap at the center of the stalk.

FIG. 7. Alfalfa yellows.

No record has been found of typical boron-deficiency symptoms in mature pea plants (a plant which has a very low boron requirement). However, in a germination test of peas from a known boron-deficient area, the sprouts were pale and stunted, and they lacked the recurved plumular bud typical of normal sprouts.

A distinct yellowing and stunting is usually characteristic of alfalfa (Fig. 7) when grown in a region in which the boron supply is inadequate. If the deficiency is slight, the leaves are light green, and as the trouble increases in severity the color changes to a distinct yellow. In marked deficiencies the leaves remain small, the growth of the tip practically ceases, and flowers are not formed. The internodes on affected plants are shortened, but the leaf petioles appear to be of normal length. This condition is usually most severe on dry fields. An application of 30 pounds of boric acid per acre has controlled the stunting and yellowing. The boron content of yellowed plants was found to be consistently lower than that of green plants, with values of 4–9 and 14–32 ppm, respectively, on a dry weight basis.

Several years ago in British Columbia, wheat that was heading failed to produce kernels, and the results of analysis of some of these heads indicated that the boron content was only 5 ppm. After treatment with boric acid at the rate of 30 pounds per acre the heads developed normally and the boron content had increased to 9 ppm. In British Columbia failure of corn to set kernels has also been corrected by the application of boric acid.

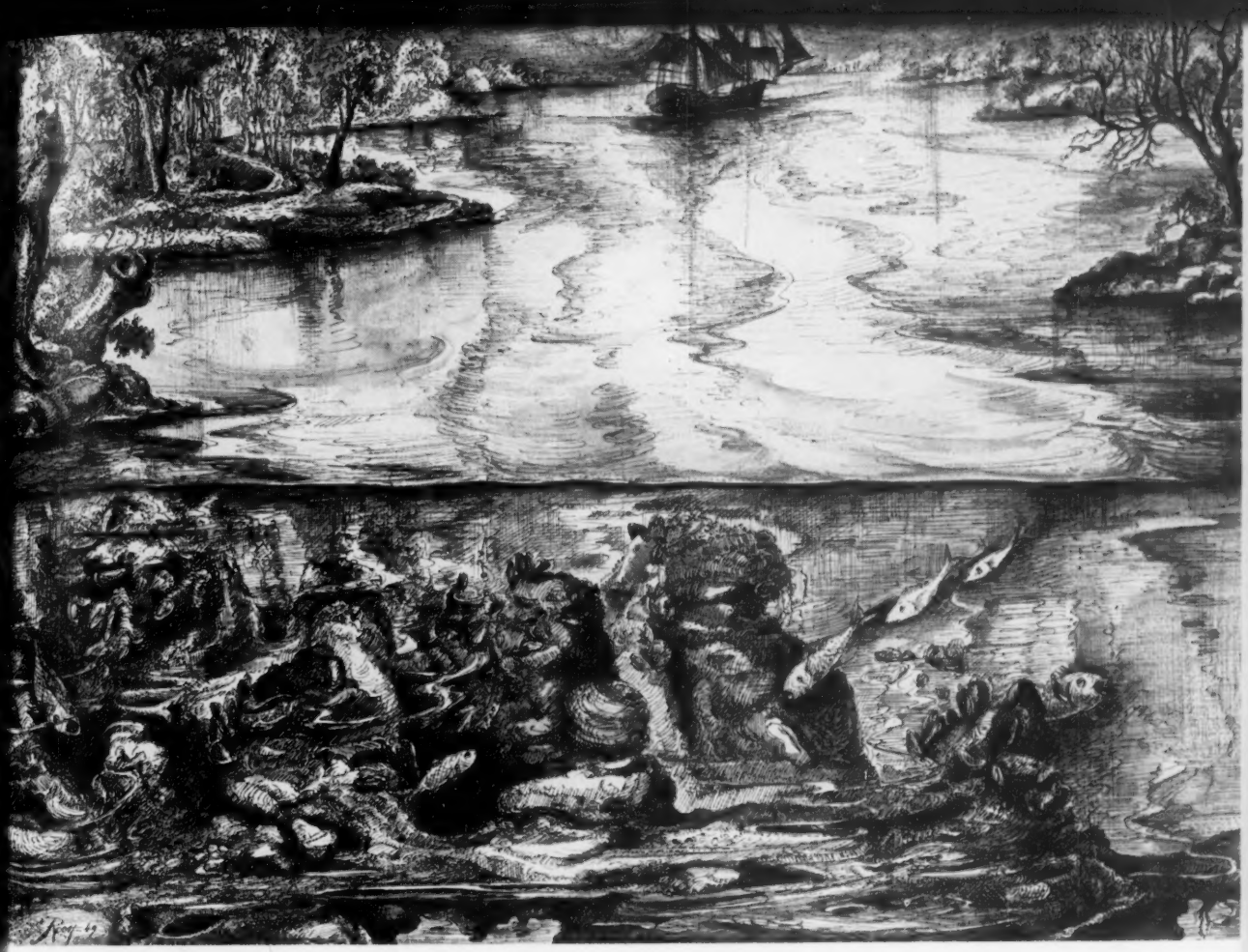
When tomato plants are grown on boron-deficient culture, growth is slow and deficiency symptoms can be found on both the fruit and leaves. The fruit epidermis may be roughened, and brown necrotic spots may be found, particularly near the stem end. The edges of the leaves may become brown, especially near the apex, and the veins may turn pinkish.

There is a great variation in the ability of different roots to absorb boron from the growing medium. Eaton and Blair believe that the rate of absorption of boron is determined by certain inherent characteristics of root epidermal or absorbing cells and also by the nature of the boron compounds that are distributed throughout the plant. They point out that the absorption and accumulation of boron do not depend wholly on the roots. It was observed that Eureka walnuts accumulated more boron in their leaves than did the Payne variety when grown under similar conditions and on the same root stock. Hansen studied the influence on root stalk of injury from excess boron in French (Agen) prune and President plum and found that of those tested almond roots are least susceptible to boron injury.

Several workers have studied the effect of various elements on the relationship, if any, that exists between the incidence of a boron-deficiency disorder and that element. Boron seems to aid in the absorption and utilization of calcium, and, in the absence of boron, less calcium but more nitrogen and potash are absorbed. It has been repeatedly noticed that, during a season of relatively low moisture, boron-deficiency troubles are more severe than during a year in which the precipitation is high.

A great deal of work remains to be done so that the function of boron in the plant may be clearly understood. There is evidence to indicate that a deficiency of boron interferes with the process of translocation within plants and that it is usually associated with a breakdown in cell structure. It may be associated with calcium in the compounds which make up the middle lamella. High calcium and high nitrogen accentuate the symptoms of boron deficiency, but the relationship between these elements is not fully understood. Further studies could profitably be undertaken to establish the effect of water and light upon boron utilization by a plant. The relationship between susceptibility to disease and mineral nutrition, especially boron, requires careful consideration. It has been shown that when carrot roots contained 43 ppm boron on a dry weight basis the plants were more resistant to bacterial blight (*Xanthomonas carotae*) than when roots contained only 19 ppm.

In concluding this discussion on the use of boron compounds in agriculture in the Pacific Northwest, it is important to emphasize the extreme complexity of the conditions that govern the use of this element. It has been through the use of extensive field and laboratory tests that so much is known today about the requirements of different plants, and this knowledge can only be accurately interpreted and used by studying the crop to be grown, the type of soil, previous fertilizer practices, and climatic variances. It is only by continuous and consistent research that real progress can be made on agricultural problems such as have been discussed in this article.



Treasures in Troubled Waters

The Plight of the Oyster

CURTIS L. NEWCOMBE

A native of Canada, the author took his Ph.D. at the University of Toronto in 1933, after which he was associated with various colleges and laboratories in the United States. In 1949 he became biologist for the California and Great Basin Drainage Basins area in the U. S. Public Health Service. Dr. Newcombe's article was prepared prior to his affiliation with the USPHS.

THREE hundred miles north of Boston the shores of the Bay of Fundy once grew myriads of soft-shelled clams, providing a livelihood for thousands of watermen. Today, the clam beds are overdug, and clammers are turning to other vocations. Such depletion of a natural resource can happen locally in a single season.

One summer, near Nova Scotia's historic Annapolis Royal on the same Bay of Fundy, husky clammers dug as many as four barrels "at a tide," while double-yoked oxen hauled the catches up the gutted beach to waiting trucks bound for the Port of Yarmouth. There, before sunset, the rich cargoes were disgorged onto a Fundy boat, and early the following morning they were sold on the Boston market.

That was a summer of bountiful harvest. Virtually every cubic foot of beach was excavated. The marketable clams were taken by the diggers; undersized ones fell an easy prey at low tide to the gulls, and at high tide to the flounders. The latter came in great schools—150 could be hooked in an hour—and it was not exceptional to find 100 little clams in the stomach of a single fish. It was a

Starfish attacking young oysters. A starfish can open even a large oyster by the strong and steady pull of its sucker-equipped "feet," found in rows on its lower surface. The starfish then inserts its own stomach inside shell and digests the meat of the oyster.

prosperous summer for the entrepreneur, the clammers, the gulls, and the flounders, but by September, from the now deserted flats, it was my discouraging experience to dig, after an hour's work, scarcely a peck of marketable clams for research purposes. On these estuarine shores the marks of depletion had been swiftly and effectively gouged. Nature's hourglass will painfully measure years before the scars of hasty exploitation can be erased.

I

The oyster, used by man for food since ancient times, is a striking example of the continuing onslaught on man's sorely needed food resources. As long ago as 50 B.C. Sallust disclosed the high esteem in which oysters were held, when he wrote, "The poor Britons—there is some good in them after all—they produce an oyster." The oyster is known to fill nearly all dietary requirements; in fact, it ranks with milk as an almost perfectly balanced food. Oysters contain far more iron than milk and approximately forty times as much copper and iodine as beef.

Unhappily, this valuable shellfish is meeting the fate of the clam and other overexploited marine animals. In addition, it is suffering from man's pollution of its environment. As one drives south through the once heavily forested Coastal Plain and Piedmont country of historic Chesapeake Bay, there unrolls a relatively denuded landscape compared with the days of "King" Carter of Nomini Hall, whose Virginia plantations in tobacco, "the coin of the realm," covered a third of a million acres. Continuing over silt-laden streams, a consequence of deforestation and improper land use, the highway crosses a river whose heavily silted waters also bear industrial waste—pollution twofold. A boat ride down the York River and a sampling of the oyster grounds of this once-famous seafood estuary complete the picture of past and present happenings in this particular Atlantic watershed. Whereas several hundred boats plied the York oyster trade as late as 1900, less than fifty can be counted now. Oysters are scarce. Little wonder that, within a stone's throw of the oyster shore, at the time of my visit they were selling for more than a dollar a dozen.

A meaningful story lurks back of these observations. On the bright side of the conservation ledger, there is the industrial plants' use of improved methods of disposal to reduce pollution, thereby removing a threat to the oyster fishery. Likewise, modern forest-management practices, minimizing siltation, are being introduced. But silt from overworked and poorly managed land is still piling up on the oyster bars in unbelievable quantities.

Cluster of "coon" oysters—result of overcrowding.

(These bars, or "rocks," are elevations above the bottom, consisting of great accumulations of oyster shells built up during geologic time. On their clean, unsilted surface the oysters of today thrive.) This deplorable circumstance points bluntly to the fact that in the Chesapeake country we are needlessly losing our fertile topsoil, and that in so doing we are extinguishing perhaps the world's greatest oyster center.

II

Like its European counterpart—the Baltic—the Chesapeake is a great inland sea. Shared by the commonwealths of Maryland and Virginia, it extends north and south 180 miles; its average width is about 8 miles, and its average depth approximately 40 feet. Numerous dendritic tributaries render the bay readily accessible to man.

When Captain John Smith arrived at the entrance to Chesapeake Bay in 1607, the condition of the oyster beds was a far happier one than today's. George Percy, in *Travels and Works of Captain John Smith*, tells of landing there on April 27, 1607, to find newly roasting oysters over a fire that had been deserted by the Indians on the approach of his party: "We eat some of the oysters which were very large and delicate in taste."

To the colonials the bay was a stable and dependable source of food. It was, in truth, the cradle of the nation's fishing industry. The attention of the Swiss traveler Francis Louis Michel was drawn by the "indescribable large number of big and little fish" in rivers and creeks of Chesapeake Bay. On the subject of oysters he wrote in 1702: "The abundance of oysters is incredible. There are whole banks of them, so that the ships must avoid them. A sloop, which was to land us at Kings creek, struck an oyster bed, where we had to wait about two hours for the tide."

By the middle of the eighteenth century, Chesapeake oysters had increased in popularity throughout the Tidewater country. The colonial historian John Pierce Middleton believes that probably the earliest record of their cultivation in the Chesapeake region is preserved in the letters exchanged by John Ridout, of Annapolis, Maryland, and John Leeds, a gentleman of Maryland's Eastern Shore:

Ridout to Leeds, ANNAPOLIS Dec. 21, 1753

As you was pleased some time since to enlarge on the Merit of a certain Gentn: residing in Your County I could not but take this opportunity . . . of expressing myself very sensible of the want of a Connoisseur of that Turn in these Parts, by whose means I might have an opportunity of removing the prejudice his Excellency [Governor Horatio Sharpe] (tho very fond of Oysters) has conceived against the insipid species with which [we] are supplied at Annapolis.

Oyster tongs and natural growth of native oysters.

Leeds to Ridout, TALBOT COUNTY Jan. 15, 1754

I thank you for your kind congratulation on Supposing me to Live in the neighborhood of fine Oysters, but since you have been pleased to take particular notice of what I but just hinted concerning my Ingenious friend, I beg Leave to Explain myself a little; He does not make that his whole business, far from it, his Schemes are of a much higher nature than for any thing that can be pickt out of the Mud. But I suppose happening to hear of a Person famous for Raising Mushrooms, a Sudden thought [Ms. torn] into his mind, why may not a man become famous for raising Oysters, since a little care and management, may make the relish of the one, equal to the Flavour of the other. . . .

—Bozeman Papers, Library of Congress.

Thus, the stage was set for developing one of the truly great marine resources. Unfortunately, this fishery has not fared well in the hands of a short-sighted generation bent on wholesale exploitation.

III

Let us examine the record. As early as the 1880s, Winslow reported that many oyster beds of Chesapeake Bay were deteriorating. An estimated 12,000 or more tongers worked in Virginia alone; today the number is only about 2,500. A total annual production in the bay of over 20 million bushels has been reduced to around 7 or 8 million, and the decline in Maryland has been about 75 per cent. It is not strange that the voices of many Tidewater conservationists and other farsighted citizens have

been raised in that pertinent query, Why? And no less strange that the price of oysters has soared beyond the buying power of the average family.

How much of the decline is due to overfishing of the natural oyster bars, and how much to misuse of the watershed? Studies have shown that overfishing and siltation operate together to cause depletion. A depleted public oyster bar is left idle, and the longer it is not "worked over" the greater the opportunity for riverborne silt to accumulate on the shell surfaces, thereby destroying the essence of its vitality. The free-swimming larval oyster requires a hard bottom of clean shells on which to fasten. From then on it leads a sedentary life. Throughout the Chesapeake tributaries silt is the oyster's greatest natural enemy. For, in the words of Bryant, "rivers are moving roads"—not only for human transportation, but also as fluid avenues of transport for millions of tons of sediment.

On depleted, and hence little-used, oyster grounds, silt accumulates rapidly and suffocates the surviving stock. Without the artificial planting of shells or other suitable materials to provide a surface for the larval oysters to attach themselves, the usefulness of this natural oyster bar ends for all time. Unfortunately, artificial planting of shells in sufficient quantities to overcome the silt is practical, in a business sense, only when executed during the early stages of siltation. Time is therefore important. It has been my observation that many of the once-productive oyster bars have already been buried deep in silt and mud, and others are in progressive stages of deterioration. The urgency of this situation is apparent.

IV

One asks: How have the changes in the Chesapeake watershed since colonial times affected the amount of silt in the bay waters? How does silt affect the capacity of water to produce food? The early settlers found a land of continuous forest; but it was short-lived, as is indicated in a letter to Benja. Woodroof, F. R. S., from the Reverend Hugh Jones, dated January 23, 1608/9. To quote:

> All ye low land [In Virg[a] and Mary[d]] is verry woody like one continuous fforrest no part clear but what is cleared by ye English And tho we are pritty closely Seated yett we cannot See our next neighbours house for trees. Indeed in [a] few years we may expect it otherwise for the tobacco trade destroyes a abundance of timber both for makeing of hhds & building of tobacco houses besides clearing of ground yearly for planting.

The fifty years immediately preceding the American Revolution witnessed the growth of great families and the extension of their land holdings. Land hunger knew no bounds, and economic security was measured in terms of land and slaves. The life expectancy of fertility in cultivated fields was but three or four years, and, as the fields were exhausted, more forests gave way to the plow. Rehabilitation of the soil was left to nature, which worked slowly to build up, but rapidly and inexorably to tear down. By the end of the seventeenth century the way was paved for malignant erosion of the Tidewater Coastal Plain, with inevitable destruction of the vital elements of the land.

Any comparison between the clarity of the Chesapeake waters two centuries ago and their opaque, lustreless aspect today provides evidence of the effects of soil erosion since colonial days. Tatham wrote about the silting up of rivers in his *Essay on Tobacco,* dated 1800: " . . . the constant sullage from the plough, and other washings of the upland counties, have impeded the navigation, which formerly held a better channel near the falls of the several rivers, but particularly James's river, where the channel is subject to frequent changes. . . ." And, indisputably, the subsequent continuation of this "sullage from the plough" has had a devastating effect on the estuarine waters of the bay. Gottschalk has estimated that over 85 million cubic yards of sediment were deposited at the head of Chesapeake Bay alone between 1846 and 1938.

Bathymetric charts show that the major area of the bay and its tributaries is less than 33 feet in depth, and that there is evidence of great depositions of silt. There is other proof, too. In the early eighteenth century Joppa Town, at the mouth of the Gunpowder River, became the county seat of Baltimore County, Maryland. It was a gay, prosperous seaport, and from its wharves ocean vessels carried agricultural products of the commonwealth's cleared lands across the seven seas.

Bunch of oysters showing growth of mussels and barnacles.

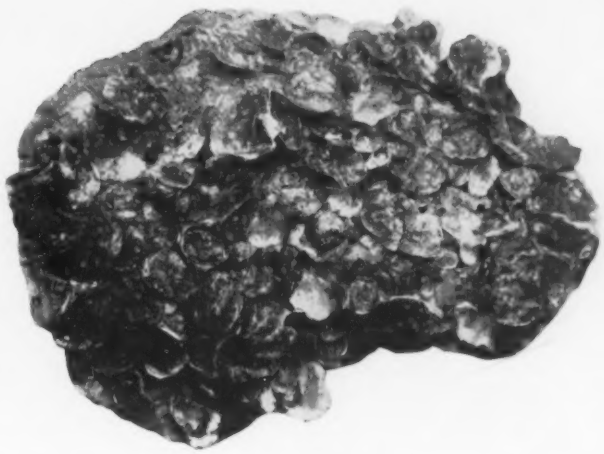
Unhappily its source of greatness sealed its death warrant. Cleared lands spelled erosion, and erosion reached the economic heart of the town. Sedimentation blocked the harbor, decline followed, and within about sixty years the county seat was moved to Baltimore. Today, the above-tide deposits extend a mile and a half below the townsite. Joppa Town is no more. In its place a delta marks the entrance to the river.

Excessive erosion has not only devitalized the Coastal Plain; it has blanketed valuable oyster rocks, built up patiently through aeons of time. It has helped to neutralize the productivity of the bay waters, negating their fertility by drawing the curtain against light. It has rearranged nature's pattern by disturbing the delicate balance of physical, chemical, and biologic factors. Not only fewer land plants, but fewer water plants—diatoms, the "grass" of the sea—are the inevitable result.

Such habitat changes affect the oyster's ability to gain a living, to reproduce and grow. They reduce the resistance of the bay waters to man's profligate handling of his inheritance. More specifically, they represent an attack on the oystermen's capital, the capacity of the water to produce high yields. With a declining capital, the interest rate, in the form of annual catches, likewise depreciates. And what happens? The oyster community becomes increasingly less resilient; it can absorb less strain, less misuse, less wasteful handling; and it takes more time to recover, not only from these contingencies, but from natural enemies and nature's catastrophes as well.

V

What is the solution to the problem? A partial answer, at least, lies in the knowledge of its causes. Compared to agriculture, oyster farming is in its infancy. Education and research in the aquatic field are only now beginning to be effective in a practical way. Oyster research has been too much confined to the laboratory, partly because of the cost of large-scale field experiments. Certain of the promising ways in which an oyster farmer might increase his production have not been adequately studied. Techniques for pest control, for improving the genetic strains of the oyster stocks, for large-scale experimental plantings designed to demonstrate the best ways of growing oysters in particular environments, are sorely needed. I have seen the population of a large acreage of oyster ground suffer a mortality of over 90 per cent from the infestation of oyster drills, or marine snails. Even more significant, one may see boatloads of drill-infested oysters being transported to pest-free



Oyster spat, five weeks old. Young oysters, or spat, settle to the bottom and attach themselves to hard, clean objects after spending a short time in a floating phase. Most commonly they attach to old oyster shells. Best results are obtained if the shells, or cultch, is planted shortly before the spat sets to the shells.

grounds in the course of oyster-farming operations. Hundreds of acres of barren mud flats bury once-productive oyster grounds to a depth of over a foot. This destructive siltation continues at an increasing cost to the oyster industry and to the consumer public. On the findings of scientific research and their successful application depends the future solvency of the oyster industry.

Efficient and informed administrative leadership is needed also. Through the years administration of fishery problems has been, in large measure, part of a political spoils system directed almost without exception by leadership drawn from other fields, woefully handicapped by a lack of knowledge of fisheries. The continuance of such governmental policy is made possible by an absence of informed public opinion. To remedy this situation educational programs are necessary. Improvement of natural oyster grounds (public rocks, available to any tonger who has a boat, tongs, and a license to take oysters in unlimited quantity) by the state, or through a change of policy whereby they may be made available to improved oyster farming, or by a combination of these two, all depend on having first and always an efficient fishery administration.

Natural oyster grounds represent the growth of millennia, and their destruction is accordingly slow; but, during the past fifty years processes leading to their deterioration have assumed startling proportions. It is time for our citizens to join hands in an effort to abolish the causes of this decline and to erect a solid framework of constructive management and administrative policy.

Loading oyster shells into bateaux for planting as cultch in oyster propagation. This oyster-shell pile at cannery contains approximately 1,000,000 bushels of shells.

Whatever doubt may exist on matters of oyster policy, it is crystal-clear that solutions to the problems raised rest on efficient machinery for research, education, and administration. Without the latter, the former two are doomed to sterility. An informed citizenry could demand a change from political subsidy to nonpolitical, educative, and investigative processes. Little wonder that progress is at a snail's pace!

Aquicultural policy makers must not lose sight of the great man-made transformations that are causing the defertilization of natural waters and the destruction of rich submerged food-producing grounds. Nor should the remedial measures now essential for recovery continue to be complacently ignored. Past experience, modern science, honest administration, and in-service education are within man's reach. He can yet seal the lid of Pandora's box.

Photographs by courtesy of U. S. Fish and Wildlife Service. Artist's concept of a Chesapeake tributary in colonial times (p. 105), with oyster bars reaching surfaceward, by Paul Roch.

The Moral Obligation to be Intelligible

NEIL E. STEVENS

Dr. Stevens (1887–1949) was, until his death last June, professor of botany at the University of Illinois, a post he had held since 1936. After taking his Ph.D. at Yale in 1911 he went to the University of Kansas and later to the U. S. Department of Agriculture, where he worked for twenty-four years. His article is based on a seminar held for a class in scientific writing at Illinois.

THAT my title is borrowed from John Erskine's penetrating essay, *The Moral Obligation to be Intelligent,* is obvious. In view of the fact that almost everything said here is borrowed (usually in direct quotation), it seems fitting to borrow most of the title also.

There is a case for, as well as one against, scientific gobbledygook. This term, coined by Maury Maverick was defined by him as "talk or writing which is long, pompous, vague, involved, usually with Latinized words" (N. Y. *Times Magazine,* May 21, 1944, 11).

The case for the use of gobbledygook by an ambitious scientist is simple and sound. It is one of the surest ways to get oneself taken seriously; that is, to impress people with one's importance and wisdom. As Aristide Briand is quoted as saying (by Andre Maurois in *Vanity Fair* for June 1931), "What the bourgeois public loves is a man who carries himself like a holy prophet and who always has the air of having just come from a funeral."

This conviction that thoughts expressed in big words must be profound and that the man who uses big words must be learned is deeply ingrained in our culture, and goes very far back. For example, in Hans Christian Andersen's well-known fairy tale of the *Nightingale,* the superiority of the artificial bird is recognized by its title, "Chief Imperial Singer of the Bedchamber," and the music master wrote five and twenty volumes about the artificial bird . . . "in all the most difficult Chinese characters. Everybody said they had read and understood it, for otherwise they would have been reckoned stupid and their bodies would have been trampled upon." Later on this same music master made a speech about the nightingale "using all the most difficult words." Again, in the fairy tale *Marsh King's Daughter,* the father stork reports in the home nest on a gathering of the learned men and sages as follows: "I can't exactly repeat the words . . . . what they said was so involved and so clever that they not only received rank, but presents too!"

The whole is well summed up—as are so many other important truths—by Gilbert and Sullivan in *Patience*:

> If this deep young man expresses himself
> In terms too deep for me;
> Why, what a very singularly deep young man,
> This deep young man must be.

I attribute this reverence for big words largely to the doctors and lawyers, who in order to protect their prestige were purposely prolix and involved. Recently, for example, an Associated Press story carried the following sentence: "He had bilateral perobital hematoma and left subjunctival hemorrhage." This was the answer given in reply to a question in court; further questioning brought out the translation "two black eyes."

Reverence for big words and long sentences enters even into our textbook writing. Three years ago members of an English Department scored a group of recognized textbooks in general botany. On Rudolph Flesch's well-known readability scale none of them was scored as being as easy as the *Reader's Digest.* Two fell in the group with the *Atlantic Monthly* and *Harper's*; six were in the group with the *Yale Review,* and two were scored as more difficult than the *Yale Review* and were indicated as requiring at least college graduate reading ability. And not only must the words be long, the book must not be too brief.

The importance of the use of big words in maintaining prestige is shown in its purest form, at least among the plant sciences, in ecology. In fact, one is tempted to suggest that ecology maintains its prestige, to say nothing of its existence, by the use of a special and rather difficult language. A text in this field published a few years ago was

called *Bioecology,* whatever that means. I have never read it. I did, however, read a review which began:

> This book is a fine example of an important and already difficult subject discussed in an abstruse, involved, pompous and thoroughly tiresome manner. Simple things are made complex, and complex things are made well-nigh incomprehensible. . . . Nor is the mounting use of coined words helpful in elucidating the text. One is led almost to believe that ecology, as understood in the Clements-Shelford biome, is the occupation of thinking up new names for old things.

Yet both authors have long stood high in the list of well-known ecologists in this country, and I have no doubt that this volume enhanced their reputations.

Recently I reread a favorable, even flattering, sketch of a contemporary worker in a field of applied botany. There was a subheading "Explains things to growers in growers' language." The text, however, read in part "in casual conversation he is so congenial that sometimes it is difficult to take him seriously." The next sentence concluded "he seems like a layman." A sad comment!

Of course we biologists have not yet approached the philosophers. An appreciation of their methods of writing by a really great biologist is taken from the autobiography of Hans Zinsser, *As I Remember Him:*

> I know that anything I may say has been better said many times. Real philosophers, however, are also very often repetitious, and get away with it because so many of them can say old things in such an incomprehensible manner that, by the time one has reread a sentence five or six times, one takes so much pleasure in thinking that one has comprehended the thought that one forgets how old it is.

Obscure writing needs no defense. Its place is securely enshrined in the traditions of our culture, in the practice of hundreds of eminent investigators, and in the approval of dozens of editors. [Anyone who wishes to improve in this difficult art can get specific directions from an article by Paul W. Merrill, entitled "The Principles of Poor Writing," published in THE SCIENTIFIC MONTHLY, 1947, **64**, 72–74.]

In spite of the weight of authority and practice on the side of gobbledygook, I should like to attempt to state the case in favor of clarity and simplicity. First, however, I wish emphatically to oppose one accusation, which I greatly resent; that is, that we *cannot* do differently. For example, Sir James Barrie was quoted some years ago in the USDA *Daily Digest* (October 26, 1936) as saying, "The scientific man is the only person in the modern world who has something new to say and does not know how to say it."

Worse still is this comment from an article by W. T. Stace, entitled "The Snobbishness of the Learned," which appeared in the December 1936 *Atlantic Monthly*: "The impression that philosophical and scientific ideas cannot be explained in plain language to plain people is also due to the fact that philosophers and men of science have not, as a rule, the wit to do it. It is due in plain terms to the stupidity of the learned men and not to the stupidity of humanity." *Of course* we can write plainly and speak plainly. Even the most learned scientist does not order a dinner, or a drink, or propose marriage in five-syllable words.

Gobbledygook Is Not Necessary

On this point I take my stand squarely with W. C. Allee, of The University of Chicago. This is quoted from his book *The Social Life of Animals:* "Despite much practice to the contrary any biological fact which concerns us can be accurately described and the conclusions from its study can be clearly expressed in relatively simple and direct language." In proof of Allee's claim I might present one of his own books. Instead, I offer two well-known works, which stand as landmarks in the development of our science. First, let us take William Harvey's *Motion of the Heart and Blood in Animals,* first published in 1628—just eight years after a few of his fellow-Englishmen landed on Plymouth Rock. This book revolutionized (I use the word advisedly) physiology. It is the very basis of present-day medicine. It defied the prejudices of several centuries. Here if ever might big words be justified. Moreover, Harvey, who was not court physician for nothing, knew how and when to use big words, as, for instance, in the book's dedication:

> To The Most Illustrious and Indomitable Prince CHARLES King of Great Britain, France, and Ireland—Defender of the Faith—Most Illustrious Prince!
>
> The heart of animals is the foundation of their life, the sovereign of everything within them, the sun of their microcosm, that upon which all growth depends, from which all power proceeds. The King, in like manner, is the foundation of his kingdom, the sun of the world around him, the heart of the republic, the fountain whence all power, all grace doth flow.

Harvey also knew when long words were superfluous. Witness his description of the results of vivisection (p. 24):

> In the first place, then when the chest of a living animal is laid open and the capsule that immediately surrounds the heart is slit up or removed, the organ is seen now to move, now to be at rest;—there is a time when it moves, and a time when it is motionless.
>
> These things are more obvious in the colder animals, such as toads, frogs, serpents, small fishes, crabs, shrimps, snails, and shell-fish.

A more striking illustration still is when he asks the approval of his colleagues of one of the greatest concepts of all biology, the beginning, if I am correctly informed, of quantitative methods in physiology: "I began to think whether there might not be a motion, as it were in a circle."

Another great piece of biological work which changed the direction of much biological thinking and is still the subject of much discussion is, of course, Charles Darwin's *Origin of Species by Means of Natural Selection* (1859). My first thought was to quote from this book also, but I decided instead to quote from a contemporary review. In March 1860 Asa Gray opened a review in the *American Journal of Science* with this sentence:

> This book is already exciting much attention. [Later in the same paragraph he says], It is exceedingly compact. . . . The volume itself—the proof-spirit—is just condensed enough for its purpose. It will be far more widely read, and perhaps will make impression [sic], than the elaborate work might have done, with all its full details of the facts upon which the author's sweeping conclusions have been grounded [sic].

Many years later this casual reference to Darwin's writing occurred in a book *The Emperor's New Clothes,* by Johan Hjort (translated by A. G. Jayne), published by Williams and Norgate, Ltd., London: "We find in Darwin's book a number of examples, described with brilliant economy of words." We could well adopt the useful rule that any biologist having anything more important to say than Harvey or Darwin should be permitted to use longer words.

Perhaps a major reason for these great men writing as they did was that they were interested in what they had to say rather than the impression their colleagues might get of their own erudition. Perhaps I will be pardoned for quoting a somewhat frivolous remark from John Mason Brown's *Accustomed As I Am* (p. 94): "Of education, no less than of conversation, the comment holds true which was once made by a Southern woman about a dinner party that had failed. 'I don't expect anyone to be interested in what I am saying,' she drawled, 'but I don't see why they can't at least pretend to be interested in what they are saying themselves.' "

I suggest that we biologists might at least write as if we were interested in what we are trying to write.

Gobbledygook Is Unwise

By unwise I mean professionally unwise—even unsafe. It is too often forgotten, particularly by the smaller men among us, that we are almost completely dependent for the support of our work—indeed, for the support of ourselves—on the good will of the general public. Unless that good will is maintained our work will be crippled—or starved to death. It takes only a moment's thought to realize this, and it is obvious to those of us who are in tax-supported institutions. It is almost as wholly true of those who work on grants from great corporations.

Let me again have recourse to quotations. First, Karl T. Compton, speaking in 1932 (quoted from *Science*), "After all it is certain that we [investigators] are enabled to carry on our work in the last analysis because of public interest and we cannot afford for any reason to overlook the necessity of obtaining public interest." Next, E. P. Lyon, dean of the Medical School of the University of Minnesota, writing in *The Sigma Xi Quarterly* for December 1936, says: "There is a serious side to this unabsorbed gorge of science. It has given our people a bad case of indigestion. It lies in the public stomach and troubles their dreams. They do not know enough to know good science from bad. . . .

"What the world needs is not less science, but more knowledge of what science is and what it can and cannot do."

Now, to quote from John Erskine, to whom I am indebted for my title. This is from the essay *The Call to Service.* He had just spoken in a commencement address of the growing gap between priest and layman, and then says this to his listeners:

> Between you and those whom you would serve through science the same gulf lies as between the priests and those they would benefit. The protest against science is not yet so loud, I grant you, as that against religion, but it is the same in kind, and it is growing. Scientists are as eager to do our thinking for us as ever the Church has been, they are just as ready to use force to make effective the truth as they see it, and they keep their scientific spirit to themselves as effectively as the priests keep their priesthood. They look upon themselves as a caste and in the name of science they presume to be dogmatic outside of their field, exactly as the priests once did. . . .
>
> We grow rich it appears in the results of science, but poor in its spirit. . . . The criticism is justified in the places where science is avowedly engaged in handing on her torch—in your college, for example, where almost all of you studied the sciences and almost none of you were suspected by anybody of being scientific.

What I have said so far applies to all fields of science. We biologists, however, are in especial danger. We are building up a highly specialized set of languages that prevent our understanding one another.

To quote again (this time from *A History of Scientific English,* by Edmund Andrews):

> Daily it is becoming clear to more of us that what disbars us is not the extreme erudition of our colleagues, but simply the mass of verbiage under which this erudition is hidden. One has only to turn to any journal of science to see that the average person is totally unable to comprehend the newer studies in any field except his very own.

Andrews then goes on to point out what astronomy, mathematics, physics, and *especially* chemistry have done to simplify their language and draws this alarming contrast:

> In contrast to these laudable efforts at simplification stands the appalling hodgepodge of nomenclature of the entire group of biological sciences. They have consistently violated every rule of common sense, besides the fundamental ones given above. Objects are named literally by the million. . . . The result is that the most encyclopedic intellect in the world cannot ever hope to scratch more than the surface of such a mass, and scholars are confined by limitation of vocabulary alone to narrow grooves. It is literally true that a member of any one of the biological divisions of learning can more easily comprehend articles on atomic physics, stellar mechanics, or highly technical chemical material than he can those of his intellectual next-door neighbors in the closely related biological group.

If you want to have the truth of these quotations really impressed on your mind, see what you can do toward organizing the followers of the various divisions of biological sciences on a national level. No wonder the physicists and chemists are pushing us around. We deserve to be pushed. We are no nearer a national organization of the type I speak of than the nations of eastern Europe are to international organization—and their task may be completed first. The difference is the same in both cases—the impossibility of understanding the other man's language.

Gobbledygook Is Dishonest

My final point is that scientific gobbledygook is plain dishonest or, if you prefer, antisocial, by which I mean immoral. My feeling is that we as members of a democracy have a moral obligation to keep the so-called layman in understanding touch with our work and our fun. We are doing what is for us the most interesting and pleasant work in the world, living at the expense of men and women who do other kinds of work. And I believe it is our business to see that they know what we are doing.

Let me call one more witness or, in this case, two witnesses:

SALESMEN OF KNOWLEDGE

By Glenn Frank

> The future of America is in the hands of two men—the investigator and the interpreter. We shall never lack for the administrator, the third man needed to complete this trinity of social servants. And we have an ample supply of investigators, but there is a shortage of readable and responsible interpreters—men who can effectively play mediator between specialist and layman.
>
> The practical value of every social invention or material discovery depends upon its being adequately interpreted to the masses. Science owes its effective ministry as much to the interpretative mind as to the creative mind. The knowledge of mankind is advanced by the investigator, but the investigator is not always the best interpreter of his discoveries. Rarely, in fact, do the genius for exploration and the genius for exposition meet in the same mind. Many Negro mammies of the South can make a strawberry shortcake that would tempt the appetite of the gods, but they might cut sorry figures as domestic science lecturers. The interpreter stands between the layman, whose knowledge of all things is indefinite, and the investigator whose knowledge of one thing is authoritative. The investigator advances knowledge. The interpreter advances progress.
>
> History affords abundant evidence that civilization has advanced in direct ratio to the efficiency with which the thought of the thinkers has been translated into the language of the workers. Democracy of politics depends upon democracy of thought. "When the interval between intellectual classes and the practical classes is too great," says Buckle, "the former will possess no influence, the latter will reap no benefit." A dozen fields of thought are today congested with knowledge that the physical and social sciences have unearthed, and the whole tone and temper of American life can be lifted by putting this knowledge into general circulation. But where are the interpreters with the training and the willingness to think their way through this knowledge and translate it into the language of the street? I raise the recruiting trumpet for the interpreters.
>
> (*Underscoring by Walter N. Jones, Vitality Mills.*)

With Glenn Frank's main thesis I am in hearty agreement. He was, if my memory serves me well, a magazine editor before he was a college president. I wish, however, to enter emphatic protest against his assumption that there must be a mediator between the specialist and the layman, although it is certainly not that I resent the interpreters. We have, as Frank says, too few of them. What I really resent is the implication that we investigators have no responsibility to the layman, or for that matter to the interpreters themselves.

While we sit satisfied with our findings and wait for the interpreter to carry our wares to the layman, years are lost. There are plenty of examples. In 1910 or 1911, when I was a graduate student, a very competent investigator, one of the most distinguished graduates in science at the University of Illinois, explained to me that he had discovered that tobacco mosaic was regularly carried from one plant to another on the hands of the workmen. It was twenty years before anyone decided that was worth telling to tobacco growers. All it would ever have taken to bring about the practical control of

tobacco mosaic in the field was to insist that the men who handled the tobacco refrain from chewing tobacco and wash their hands. I am by no means asking that investigators join the extension workers, merely that they tell their stories in as clear and readable a form as possible.

Now I wish to indulge in what may seem a digression—but which I believe to be very much to the point. My subject, in case it has been forgotten, is "The Obligation to be Intelligible." I feel very strongly that we are misleading our fellowmen, separating them from us, and, incidentally, indulging in some unwise self-deception by insisting that we have something special—called a scientific method. I seriously doubt whether the carefully organized presentations we find in scientific journals actually represent our work.

The first person to call this idea to my attention was Professor L. J. Stadler, of the University of Missouri. We met in a day coach one afternoon, and he unburdened himself somewhat as follows: "I doubt whether these papers we publish really represent our work and the way we do it. I suspect that actually we indulge in all sorts of short cuts and reach our conclusions, or what we hope will be our conclusions, then go back and do the work, and what we present is not much more than a rationalization." I quote from memory, perhaps incorrectly, but later I found the same idea in other places. For example, Emerson in his essay on *Nature* (1844) says, "Every known fact in natural science was divined by the presentiment of somebody before it was actually verified."

Here I would like to quote from President Conant of Harvard (*Science,* 1944, **99**, 87–94) in "The Advancement of Learning in the United States in the Post-war World." Regarding this point he says:

> Considerable nonsense is often talked about applying the scientific method to social problems. What is this scientific method? The usual philosophic inquiries into the question seem to me a bit unreal when one surveys the range of methods actually employed by sciences as remote from each other as geology, systematic botany, organic chemistry and mathematical physics. Perhaps science is after all only organized common sense, preferably derived from experiment and preferably organized on a quantitative basis.

It is conceivable that the Harvard committee that prepared the excellent and widely read book entitled *General Education in a Free Society* was aware of President Conant's ideas. At any rate, there is incorporated (p. 157), in connection with the discussion of elementary teaching of science, the following forthright statement on the same subject:

> Even at this most elementary stage the student should become familiar with the direct appeal to nature which is the heart of science. . . . In reality the scientific method, of which so much is spoken for both good and ill, is whatever means may be appropriate for solving problems in the natural environment. The working scientist brings to bear upon these problems everything at his command—previous knowledge, intuition, trial and error, imagination, formal logic and mathematics—and these may appear in almost any order in the course of working through a problem. The nub of the matter is that the problem be solved. One may go back afterward to analyze what has occurred, and then may generalize it and put it in the form of a logical sequence. But that is not how the thing actually happens; . . . Nothing could be more stultifying, and, perhaps more important, nothing is further from the procedure of the scientist than a rigorous tabular progression through the supposed "steps" of the scientific method.

In other words, we prefer to get our results through experiments, and we prefer to set up our experiments in such a way that they can be analyzed statistically and appear sound. However, in case we cannot do either, we do not pick up our marbles and go home crying; if the problem seems worth while we stay in the game and use any methods that come to hand and appear to be useful. If this basic fact were made clear to the general public, we might well draw back to our support the great army of amateur workers who could and would be so useful to us. I do not mean by that that 10, or even 1 per cent, of our citizens should have betatrons in the back yard. But it seems to me that they would greatly enjoy working with us and that they could be extremely useful in making observations on a variety of important and significant scientific problems in the biological field.

Returning to the point from which we started, we might call as a final witness, Mark Twain, who is reported to have said, "The difference between the right word and the almost right word is the difference between lightning and the lightning bug."

World Soybean Production in 1948

MERNA IRENE FLETCHER

Miss Fletcher, who has been assistant professor of geography at the University of Missouri since 1943, became interested in soybean research while doing graduate work at Columbia in 1939–40. Her article is one outgrowth of her studies, complete results of which will eventually be published as part of the "Centennial Studies Project" of the American Geographical Society.

THE scarcest food commodities in the world today are proteins and fats. In the soybean we find "a commodity which contains, per bushel, three times as much protein, ten times as much fat and three times as much valuable mineral as wheat. It contains four times as much protein, over four times as much fat, and nearly four times as much valuable minerals as corn." Soybeans are the world's cheapest source of edible oil today, and the year 1948 established a new world record in soybean production of approximately 575 million bushels.

Early History of Soybeans

As is the case with many other food plants, the early history of the soybean is lost in obscurity. We know it as an annual, summer, leguminous plant, indigenous to southeastern Asia. Ancient Chinese literature reveals that the soybean was extensively cultivated and highly valued as a food for centuries before written records were kept. Other historical records indicate that it is one of the oldest crops grown by man. The earliest written record is contained in a *materia medica,* describing Chinese plants, written by the Emperor Sheng Nung in 2838 B.C. This *materia medica* contains over 300 remedies made from the soybean. In addition to being considered the most important cultivated legume, it was also one of the five sacred grains (rice, wheat, millet, soybeans, barley) essential to Chinese civilization and was called "the little honorable god."

In the prewar years, China led all nations in acreage and production, and Manchuria, commonly known as the "Land of the Soybean," was a close second. Hence, in the past, the soybean was grown to a greater extent in Asiatic lands than anywhere else in the world.

North American Production

Forty years ago the soybean was scarcely known outside the Orient. The conquest of America by the plant, after thousands of years in the Orient, has been the greatest in the history of all known plants. From being a curiosity in the United States, the soybean has become the second largest cash crop in the extensive corn belt, and this country advanced to the position of the world's second largest producer in 1948.*

Interest in soybean products—oil and meal—spread rapidly in the Western Hemisphere after World War I. Experiments have been carried on for many years in nearly all American countries, but little progress in commercial production has been made except in the United States and Canada.

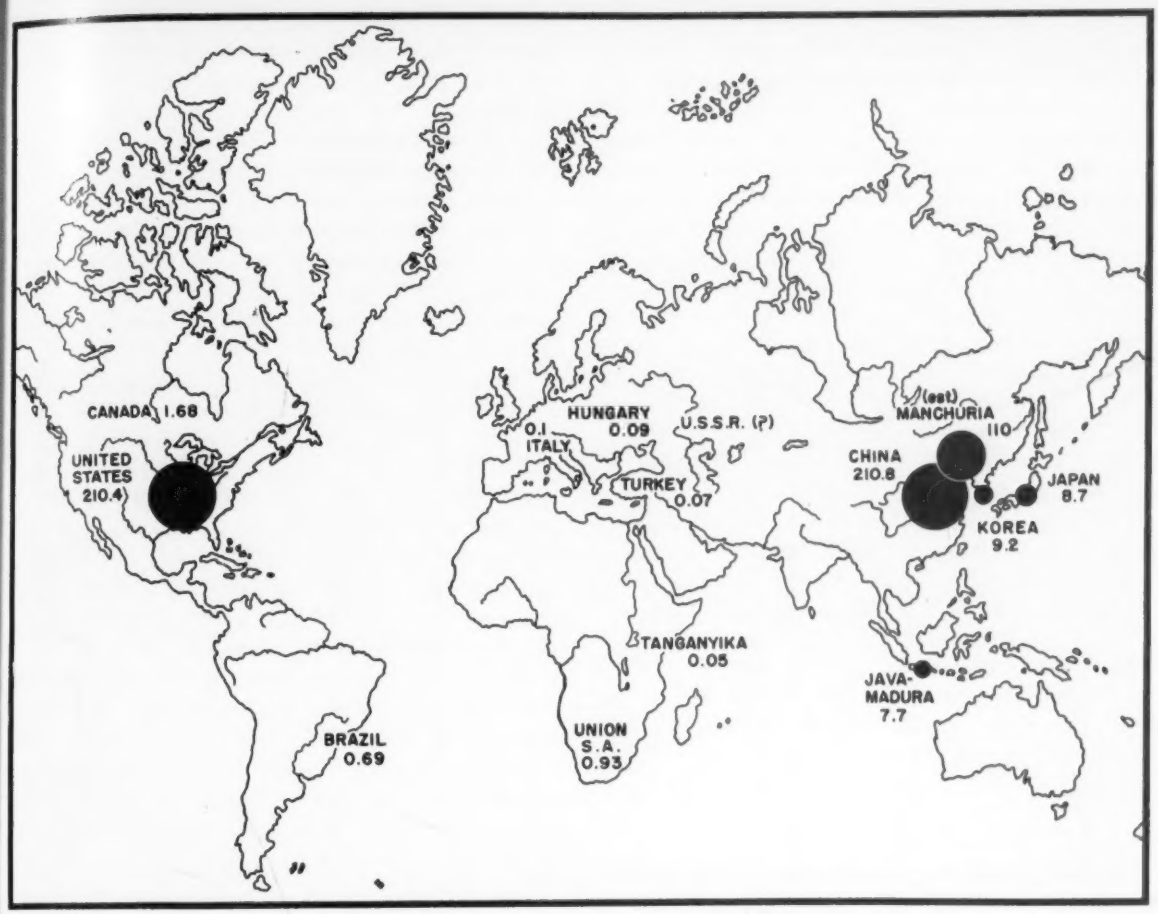
The phenomenal increase in soybean acreage and production in the United States since World War I may be be attributed to the following factors:

1. Improved varieties.
2. Mechanization of the crop.
3. Value as a cash grain crop.
4. Resistance to disease.
5. Dependability under adverse weather conditions.
6. Adaptability to cornbelt rotation practices.
7. Abundant food and industrial markets.

World War II brought changes in commodities, markets, and in interests. Progress in soybean research and production was further stimulated, so that many developments which normally would have covered a twenty-five year period were concentrated in a period of four years.

There are now three rather clearly defined re-

* W. J. Morse, principal U.S. agronomist, Beltsville, Maryland, in May 1948, said: "The U.S. now occupies first place in world production." Statistics as of January 1949 show this to be true. Dr. Morse has traveled extensively in China and is a foremost authority on soybeans.



World soybean production, 1948. (Millions of bushels.)

gions of production in the United States. The north-central region, corresponding roughly to the corn belt, constitutes the major one. A secondary region in the upper Mississippi delta country includes portions of Arkansas, Louisiana, Tennessee, and southeastern Missouri. Another secondary region, known as the middle Atlantic, includes North Carolina, Maryland, Virginia, Delaware, and parts of adjacent states.

Eight states—Illinois, Iowa, Indiana, Ohio, Missouri, Minnesota, Kansas, and Michigan—produced more than 90 per cent of the 1948 U. S. crop of 220,201,000 bushels. This is the largest crop of record. (The 1946 crop exceeded 201,000,000 bushels, but the 1947 one dropped to 183,600,000.) The record crop was brought about by exceptionally high yields, since the acreage was about 8 per cent less than in 1947. The season was also very favorable both for planting and harvesting. The Atlantic states as a group had slightly lower yields than in 1947, owing to dry weather early in the season and a wet period in October; the delta states showed greatest gains in production and yields; Arkansas reported the largest increase, with 1948 yields per acre nearly 5 bushels higher than in 1947.

Soybeans now supply a little less than half the vegetable oil and 60 per cent of the vegetable proteins produced in the United States. On the basic cash crop list for 1948, soybeans held fifth place and were valued at $519,000,000. They were preceded by corn ($4.4 billion), wheat ($2.7 billion) cotton ($2.7 billion, including cottonseed), and tobacco ($713 million).

Canadian farmers also harvested a record crop in 1948 of over 1,683,000 bushels from 94,000 acres. Because of unsuitable climatic factors, Ontario is the only province now in commercial production, and within Ontario production is concentrated in the southwestern part of the province, across the Detroit River and Lake Erie from Michigan and Ohio, respectively. Extensive research is under way at the Ontario Agricultural College and the Federal and provincial experimental farms to

develop soybean varieties suitable to Canadian climatic and soil conditions.

Canada is deficient in edible vegetable oils, and when imports were cut off during the last war the country turned to growing oilseed crops such as rape, sunflowers, and soybeans. Of these, soybeans proved by far the most successful. The 1948 crop supplied about 20 per cent of domestic edible oil needs, and soybean oil represents 30 per cent of the total liquid edible oil consumption at present. The 10 per cent not provided by domestic production is imported from the United States. During the 1947–48 crop year (August–July) Canada imported 5,876,000 pounds of soybean oil (653,000 bushels in terms of beans), 2,800,000 pounds of soybean flour, 24,200,000 pounds of cake and meal for stock feed, and 1,075,000 bushels of beans—all from the United States except for a few beans from China for experimental work.

Brazil is the only other country in the Western Hemisphere producing soybeans commercially, but until the past three years production was negligible. The 1948 crop, which totaled 690,000 bushels, represents an increase of more than four times the 1946 crop of 147,000 bushels. Greatest production occurs in the southern state of Rio Grande do Sul in the temperate portion of Brazil.

European Production

Because European scientists have long realized the high nutritional value of soybean protein and fat, Europe has been one of the principal importers of soybeans, first from Manchuria and, more recently, from the United States. Actual production of soybeans in Europe is negligible. The 1948 figures show a crop of 90,000 bushels for Hungary and 103,000 bushels for Italy. No 1948 figures are available for Bulgaria, but in 1947 the figure was 240,000 bushels. Other European countries with some acreage are Yugoslavia, Rumania, Czechoslovakia, and European USSR. The last-mentioned has carried on extensive research in the development of adapted varieties and in utilization of the crop. Principal areas of production are the Ukraine, Moldavia, and limited regions in the Caucasus. The last figures available (1935–39 average) for the USSR gave her production as 5,805,000 bushels. Some progress has also been made in the development of adapted varieties in Sweden, Poland, and the Netherlands.

During World War II Germany published an army cookbook with nearly 300 recipes in which full-fat soy flour was used to supplement eggs, meat, milk, and cheese. After the war a soybean congress was held in Weisenburg, in Bavaria, at which emphasis was placed on the soybean as a source of food, and all phases of raising soybeans and their usage were thoroughly discussed. Three experimental stations have agronomists working with soybean varieties. This may indicate that the soybean will come to play an important part in the German agriculture of the future. Germany already has several plants for processing imported beans. A soymilk that is on a par with cow's milk has been perfected at Hamburg, and is being investigated as a source of milk for undernourished German children.

France has six strong organizations interested in the promotion and use of soybeans in human and animal nutrition, for the French people have always been heavy users of oils and fats, and currently they are very short of these products. The country needs about 50 per cent more fats and oils for human consumption than she now receives if she is to bring the national diet to anywhere near prewar levels. At Bordeaux the firm of Grande Huilerie Bordelaise processes beans and manufactures soy products. French oilseed processors are particularly interested in developing the crop in the French colonies.

Britain, like all Europe, is extremely short of proteins and fats. As a result, the British Ministry of Food has encouraged the growing of soybeans, and experimentation is going on with varieties from Yugoslavia, Manchuria, Canada, and the United States. In 1948 the Ministry of Food, to attract manufacturers, set the high price of £60 per ton for soybeans. A principal paint company of England is doing everything it can to encourage production in South Africa in order to augment its supply of paint oil.

Prewar Europe imported between 30 and 40 million bushels of soybeans annually—mostly from Manchuria—for processing in plants located in many countries; the source no longer being available, many of the mills are now idle. Recovery of this industry is vital to over-all European recovery, and these mills will become markets for United States soybeans, providing an even larger market in the future. Since the close of the war considerable quantities of beans and millions of pounds of soy flour have been shipped to European countries. The director of the Fats and Oils Division of the U. S. Department of Agriculture believes that a fair estimate of U. S. exportable surpluses for 1949 will show that they will nearly equal the effective demand, but both surpluses and demand are subject to variations because of changes in government policies and weather conditions.

Production in Africa and Australia

Soybeans are an unfamiliar crop to most African farmers, although experiments have been going on over a period of years in nearly all regions of the continent. Successful results have been obtained in Gambia, Egypt, Nigeria, the Gold Coast, the upland, midland, and coastal districts of Natal, and the corn- and cotton-growing districts of the Belgian Congo. At present, however, only two countries appear interested in growing the crop commercially. In 1948 Tanganyika produced 53,000 bushels, of which 35,000 bushels were designated for export, presumably to the United Kingdom. The Union of South Africa had an output of 933,000 bushels in 1947, but no figures have so far been released for 1948. The fact that only 56,000 bushels were produced in 1946 would seem to indicate an increasing interest in the crop. Both British and French oil-processing companies are interested in promoting the crop in their colonies.

Australians are becoming interested in the crop, as manifested by the letters written U. S. authorities requesting all available information, but cultivation of soybeans is still in the experimental stage. The 1947–48 crop year was the first recorded production and amounted to only 500 bushels. The crop is confined to certain districts in Queensland, New South Wales, and Victoria.

Production in Asia

To return to the "home" continent of soybeans, the Asiatic crop for 1948 was larger than any in recent years but not equal to prewar production except in China and Turkey. The principal areas of production in the Orient are China, Korea, Manchuria, and Japan. China (excluding Manchuria) is the world's largest producer (unless the January 1949 figures prove the United States to be) and consumes the greatest quantity as food. In China proper (22 provinces) the 1948 crop of 210,820,000 bushels was the largest since the middle 1930s, although the acreage was reduced 3 per cent over 1947. To quote Dr. W. J. Morse,

> In China the soybean is one of the leading and most ancient of crops, ranking fifth in extent of culture and occupying about 9 percent of the total cultivated area. Although grown everywhere in China, about 60 percent of the soybean acreage and production is confined largely to three northern provinces—Shantung, Kiangsu, and Honan. China consumes practically all its production, estimates indicating 55 percent for food, 27 percent for oil extraction and other purposes, 10 percent for stock feed, and 8 percent for planting.

Recently returned from the Orient, Allan K. Smith, of the Northern Research Laboratory of the U. S. Department of Agriculture, reporting on "Oriental Uses of Soybeans" in the *Soybean Digest*, says:

> Shantung province is the heaviest producing area in China and soybeans are grown there as the principal crop. This province is credited with 70 percent of China's production, Honan with 20 percent, with Shansi province next in order. This production rate probably refers to commercial seed production; any estimation of production for home food use would be difficult to make (April 1949, p. 31).

The China Vegetable Oil Corporation of Shanghai is the most progressive and the largest oil-processing company in China, with branches in Tientsin, Nanking, Hangchow, Canton, and other cities. The plant at Shanghai has refining machinery, though oil for domestic consumption in China is unrefined.

Manchuria, in prewar years, was a close second to China in acreage and production. The title "Land of the Soybean" was no doubt derived from the fact that Manchuria was the world's greatest exporter—normally exporting from one third to two thirds of her production. Statistics for 1948 are not yet available, but an estimate would place the production figure at somewhat less than the 119,000,000 bushels produced in 1947. Production and export were first reduced by the disturbed political situation, and recently, because of difficulty in marketing the beans, there has been a tendency to divert to food grains land normally planted to soybeans. Typhoons and floods also reduced the 1948 crop.

Normally the soybean crop occupies 25 per cent of the total cultivated land and is a dominating factor in the life of the country. As a cash crop, it provided fully half the total volume of freight handled by Manchurian railways. Exports before the war amounted to 80–90 million bushels annually, with western Europe as the largest market. In 1939, two thirds of the crop, or 103,069,000 bushels, were exported; 15–20 per cent was utilized for food, feed, and planting; the remainder was processed for oil and oil meal. For the crop year 1947–48, only 532,000 bushels were exported. Europe felt this loss as keenly as Manchuria.

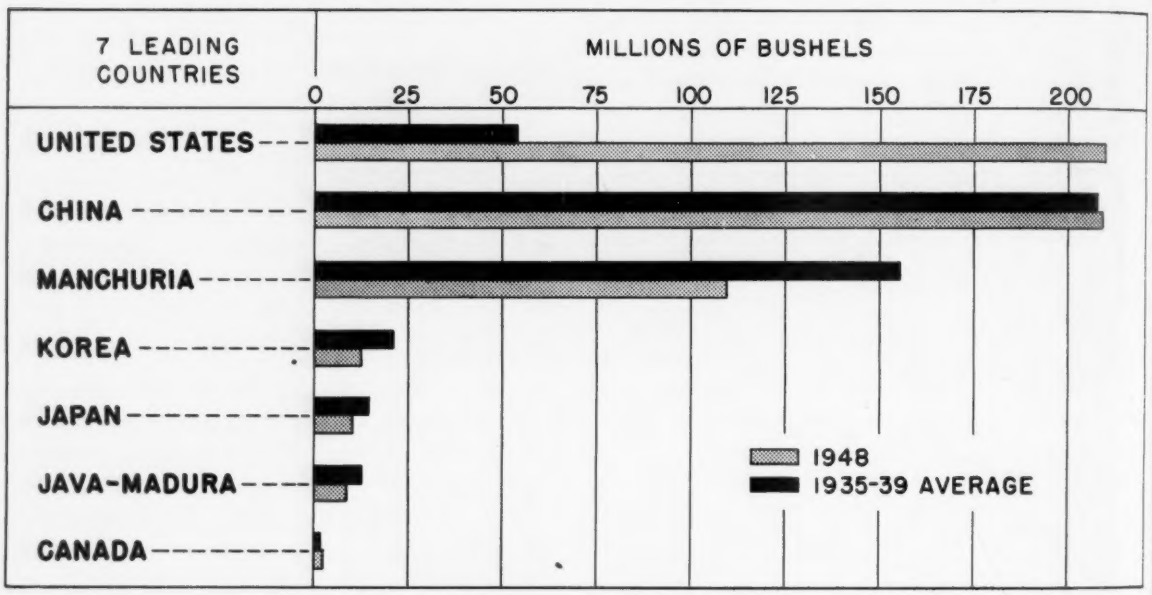
Among Asia's soybean-producing countries, Korea now occupies third place. Normally production is confined largely to the central and northern areas, but south Korea, chiefly cotton and rice country, showed a marked increase in 1948. The entire crop produced in south Korea is now utilized within the country; about 40 per cent is consumed on farms, and a large percentage

is used to manufacture edible products, but there are no processing plants as we know them. Some oil is pressed out in the home or in the farm village to be used for cooking purposes. The 1948 crop of almost 9.3 million bushels in south Korea was nearly twice that of 1947. No figures are forthcoming from Russian-occupied Korea.

Japan, although a large producer of soybeans, has consumed her own production and imported large quantities of seed from Korea and Manchuria. Between World War I and World War II production decreased somewhat, and more emphasis was placed on rice. Since 1945, production has increased annually but has not yet reached the 20 per cent of the cultivated area. Acreage and production have increased markedly since 1926, especially in the Khabarovsk territory."

Soybeans have been known in India for more than a century and may have been obtained originally from China through Tibet. The crop is grown largely in the Darjeeling district of North Bengal, in Nepal, and in Bhutan, but the total area under production in this region is only 2,000 acres. Some soybeans are grown in the Punjab, Behar, Orissa, Assam, the Central Provinces, Madras, Bombay, and Kashmir, according to Sasanka S. De, of the Indian Institute of Science.

In 1932 Mahatma Ghandi gave impetus to pro-



Soybean production for leading countries.

prewar average. The 1948 crop amounted to 8,760,000 bushels. A $60,000,000 trade agreement between Japan and Siam, announced by General Douglas MacArthur, calls for about $7,000,000 worth of railway rolling stock to be exported by Japan in exchange in part for soybeans from Siam. Between the end of the war and October 10, 1948, Japan imported 43,599 tons of soybeans from the United States, by which means she was able to supply her minimum needs. In 1946 a Japan Soybean Association was founded for the purpose of "increasing soybean production, improving varieties, and increasing the national utilization of soybeans."

According to Dr. W. J. Morse, "In the Soviet Far East the soybean is said to be one of the chief industrial crops and in some districts to constitute

duction by advocating increased human consumption. The Indian Institute of Science at Bangalore, the Nutrition Research Laboratories at Coonor, and the Indian Research Fund Association, with centers at Coonor, Dacca, Bombay, and Lahore, are all carrying on research in the nutritive value of soybeans for improving the Indian diet. The Food Department of the Federal government has constituted a committee for increasing the production of the bean and popularizing its use. It is believed the vegetable type of bean may prove more successful than the field or commercial types, with which experiments were carried out earlier.

In the Netherlands Indies (Java and Madura) production and exports still have not reached prewar levels. From 1936 until the war beans were exported to the Netherlands. From the low output

of 2,200,000 bushels in 1945 the area has gradually recovered to 7,716,000 bushels in 1948. Drought and rodents reduced the anticipated yield, so there was no exportable surplus for 1948.

Turkey is the only other Asiatic country producing soybeans commercially. Export versus production figures indicate that they are grown there chiefly for export. Of the 1947 crop of 55,000 bushels, 36,000 bushels were exported; the 1948 crop increased to 70,000 bushels.

The Office of Foreign Agricultural Relations, of the United States Department of Agriculture, places the world's 1948 soybean output at approximately 575,000,000 bushels, or about 30,000,000 bushels more than the previous record crop of 1946. Nearly 75 per cent of the world's crop is now produced in the twenty-two provinces of China proper and in thirty states of the United States. In the case of China, at least 60 per cent of the production is restricted to three northern provinces —Shantung, Honan, and Kiangsu. In the United States, 80 per cent of the total production is concentrated in Illinois, Iowa, Indiana, and Ohio.

Manchuria, Korea, Japan, and the Netherland East Indies together account for another 135,000,000 bushels, or more than 20 per cent of the total figure. This leaves less than 28,000,000 bushels of production for all the other countries mentioned in this report, plus minor producing countries for which no statistics are obtainable at present.

Asia, as a continent, still maintains its leadership with a total production of 346,569,000 bushels, but North America with its recent rapid increase in production now holds a strong second position.

As for the world's two leading producers, China's most valuable gift to the Western world has resulted in the United States becoming her greatest competitor in soybean production. The 1948 figures for the two countries are so close that only the future will show which will emerge definitely as the world's foremost soybean producer.

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Petroleum Reservoir Mechanics

L. E. ELKINS

The author is production research director of the Stanolind Oil and Gas Company, of Tulsa, Oklahoma. His article is based on a company publication entitled "The Role of Physics and Chemistry in Petroleum Reservoir Mechanics Research and Practice," which was designed to explain the nature of the problems currently being faced in this relatively unexplored field.

IN petroleum technology, reservoir mechanics may be defined as the measurement, correlation, and interpretation of all factors related to the reservoir and its contents which directly or indirectly lead to the following determinations:

1. Volume of fluids in place.
2. Availability of natural energy that can be utilized for the expelling of oil and/or gas from the reservoir.
3. Optimum number of wells and their proper location to deplete the reservoir most economically.
4. Maximum rate of production from the reservoir that will permit the optimum utilization of the available reservoir energy.
5. Proportion of the total oil and gas in place that can be recovered with the utilization of the natural reservoir energy.
6. Feasibility of supplementing the natural energy with secondary sources of energy or other processes directed toward the removal of oil and/or gas that cannot be removed by the application of the natural forces.

Evolution of the Concept of Reservoir Mechanics Engineering

In the early days of the oil industry, the most glamorous event was the bringing in of a gusher. A wildcatter increased his stock-in-trade by the height that a column of oil would flow over the top of a derrick. Reservoir engineering was unheared-of. Nature took care of the entire problem when a drill bit penetrated an oil- or gas-bearing reservoir, and the main problem was getting this oil captured through the use of flow lines and tanks.

The economic aspect was rather simple—wells could be paid out in a few days, and, once the gusher was turned into tanks, the wildcatter had enough money to start drilling offset wells, and a rip-roaring development program ensued. As soon as the reservoir pressure fell off significantly, the ever-aggressive wildcatter jumped into some new area. No one cared about what was left behind; the geologist had several more attractive prospects ready to move in on.

Then, in 1931, came East Texas—a tremendously large field with an extremely high reserve and an unlimited producing capacity as standards of conservation existed at that time. For several years prior to 1931, a few scientists had been preaching conservation in the terms of advantages of restricted production. Notable in this respect is the fact that, through cooperative effort, operators in the Van, Yates, and Hobbs pools voluntarily worked out conservation programs without the direction of regulatory bodies. But, when East Texas hit, realization of the necessity for conservation was accentuated by the marketing problems, and the oil industry was persuaded that it had been overlooking a real bet—so proration entered the picture. Someone well defined a petroleum engineer as "a hybrid geologist created by proration."

Petroleum engineers, who later split off into two groups, production engineers and reservoir mechanics engineers, gradually became aware of a number of factors concerning reservoir mechanics:

1. They began to measure and interpret the significance of gas dissolved in oil in the reservoir under existing pressure and temperature conditions. Likewise, they explored the physical chemistry of retrograde condensation for application in gas reservoirs.
2. They discovered the presence of interstitial water in the reservoir, and its effect upon total oil and gas in place became well recognized.
3. Improved techniques were developed for measuring porosity and permeability, followed by the determination of the ability of oil and gas and water to move through reservoir rock in the presence of either of the other two fluids at different degrees of saturation.
4. It became apparent that oil, gas, and water in place under a certain tract were not necessarily confined there and that, under certain conditions in reservoirs, regional migration of fluids from one part of a reservoir to another during the process of production is a paramount consideration.
5. The heterogeneity of pore space structure and its very important effect upon both the efficiency of the recovery mechanism and the methods of producing and completing oil wells gradually became one of the principal problems of the reservoir mechanics engineer.
6. The significance of surface tension between liquids and their confining rock surfaces and of interfacial tension between the different fluid surfaces in the reservoir became recognized as one of the paramount factors that will have to be reckoned with in reaching maximum recovery from different types of reservoirs.

Thus, the "hybrid geologist," who first differentiated into the petroleum production engineer and

the petroleum reservoir engineer, has undergone further differentiation. The ultimate exploitation of our oil reservoirs under the guidance of the reservoir mechanics engineer requires specialization in physics, chemistry, and chemical engineering.

Common Types of Petroleum Reservoirs

With this background, let us look at the different types of reservoirs with which the reservoir mechanics engineer deals. Generally speaking, oil and gas accumulate in porous rock where the formation rock lies over a "structural high," or where there is a porosity "pinch-out" in a formation lying on the flanks of a structural high (Fig. 1). Oil and gas collect in such formations because they are less dense than water, which originally occupied this pore space. As they accumulate in any reservoir rock, the oil and gas gradually migrate updip through these formations, displacing most of the water as they are trapped in the reservoirs of the type just described.

These reservoirs may be of four general types from the reservoir mechanics viewpoint:

1. *Depletion type:* Broadly speaking, this kind of reservoir is initially filled with oil, with all or most of its associated gas dissolved and contained within the liquid phase. For production of oil, the entire source of energy comes from the vaporization of this gas and its action in driving oil toward the producing wells. This type is characterized by the relatively rapid bottom-hole-pressure decline, a low recovery efficiency in the order of magnitude of 25 per cent, and the gas-oil ratio build-up to approximately seven times the initial solution ratio as it approaches its depletion stage. The total volume of gas dissolved in all the oil in the reservoir is used to recover 15–25 per cent of the total oil in place.
2. *Segregation gas cap drive:* In this type, there is an oil phase containing dissolved gas beneath a gas cap. Upon production, energy is supplied partially by the expanding gas cap and by vaporization of gas out of the oil. The bottom-hole-pressure decline is less relative to the depletion-type reservoir, and the extra volume of gas in the reservoir may increase recovery to approximately 35 per cent of the oil in place. The gas-oil ratio near the depletion stage rises in this example to twenty-five times the initial solution ratio. This illustrates how the movement of greater volumes of gas through a reservoir with some degree of pressure maintenance will strip more oil from the reservoir rock and thereby significantly increase oil recovery.
3. *Water-drive type:* In this type of reservoir oil is found in structures similar to those in the depletion or segregation reservoirs, but upon production the withdrawn oil and gas are replaced by water encroaching from the edge or bottom. Water as the displacing medium tends to maintain pressure (Fig. 2). The gas-oil ratio remains close to the solution ratio, and for the most part the dissolved gas remains in solution until the oil is in the well. It aids in lifting oil to the surface, but it does not significantly enter into the recovery mechanism. The typical recovery efficiency is 70 per cent as compared to the 25 and 35 per cent in the previous examples.
4. *Combination type:* Many oil field reservoirs are a combination of two or three of the different types described, and it is in the combination type that the reservoir mechanics engineer finds the most complex problems.

Significant Factors Entering Into Reservoir Mechanics Engineering

Type of porosity and permeability in reservoir rock. There are three principal types of reservoir rock: unconsolidated sand, consolidated sand, and limestone.

In unconsolidated sand, the pore channels are *relatively* large, but have very irregular shapes as they surround the grain contact points. The significance of pore channel shapes will become recognized more and more as the industry looks to the reservoir mechanics engineer to lower to an irreducible minimum the oil left in the reservoirs that geologists have discovered. Generally speaking, unconsolidated sandstone pools are benefited by an active water drive, and the relatively high percentage of porosity and high permeabilities make this type of pool very attractive to the operator. In addition, the reservoir engineering problems in reservoirs involving unconsolidated sand are not

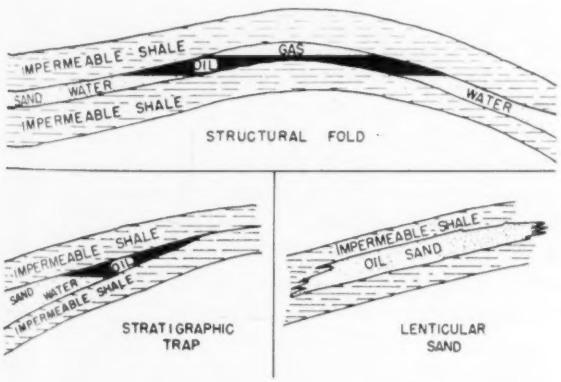


FIG. 1. Some typical geological structures promoting oil and gas accumulation.

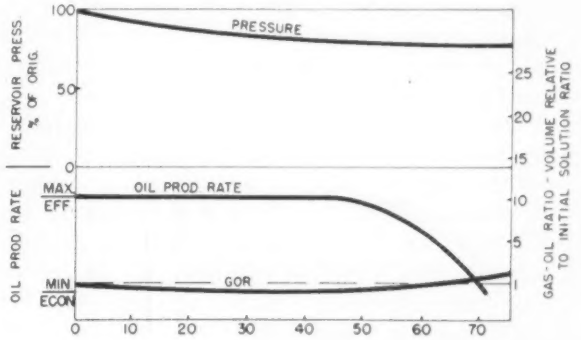


FIG. 2. Typical performance, water-drive reservoir. Cumulative oil production, percentage of oil in place.

quite as complicated as those in other types.

Consolidated sandstone contrasts with unconsolidated sand in that part of the pore space originally present between sand grains has become cemented, resulting in reduction in porosity and permeability. This type of reservoir does in some instances have an active water drive, but in many more instances it is characterized by the other types of reservoir mechanisms. The cementing process often creates marked variations in porosity and permeability in the same sand bed. Such variations are of real significance to the reservoir mechanics engineer in establishing maximum efficient producing rates, particularly in reservoirs subjected to water invasion. The problems become more complicated with increasing degrees of cementation.

Except in cavernous limestone, the limestone pools are characterized by depletion-type performance. The porosity and permeability are of a relatively low order, and their nature is entirely different from either sand type of reservoir rock. The extreme irregularities in pore space size and shape complicate the recovery problem in dolomite reservoirs. Lowest recovery efficiency is in this type of pool, and by the same token the dolomite pools offer the greatest challenge to the development of procedures for removing the 80-odd per cent of the oil in place that we are not now recovering.

Physical characteristics of reservoir oil. The vaporization characteristics and shrinkage of reservoir oil in place brought about by a reduction in reservoir pressure are important factors that must be measured, interpreted, and used by the reservoir mechanics engineer. For example, if the reservoir mechanics engineer is analyzing a reservoir in which there is a high shrinkage oil, he must realize that reservoir oil saturation will be decreased by two factors; namely, the withdrawal of oil from the reservoir and the shrinkage of the remaining oil in the reservoir brought about by pressure decline. Shrinkage of oil in the reservoir is very significant in that it substantially reduces the productivity of wells and furthermore limits the recovery efficiency of oil in place if it is not in some way controlled through the artificial maintenance of pressure.

Relative permeability. The use of permeability measurements on reservoir rock has evolved considerably since its first application in petroleum engineering. This was brought about primarily by the recognition that the flow in most reservoirs involves at least two fluids, oil and gas with interstitial water immobile, and in other instances the simultaneous flow of all three fluids (oil, gas, and water). The research laboratories have developed means for measuring the permeability of different types of reservoir rock to one fluid in the presence of one or two other fluids. There is a rapid *decrease* in permeability to oil with a slight increase in gas saturation. This increase in gas saturation is brought about by a pressure reducton promoting liberation of gas from the oil and a concomitant shrinkage of the oil. Knowledge of these factors allows the reservoir mechanics engineer the freedom accurately to predict the oil- and gas-producing capacities of individual wells and the reservoir at large. This type of information is critical in planning the proper exploitation of reservoirs and in determining the volumes of gas to be processed in a gasoline plant or pressure maintenance plant from time to time during the life of the field.

In a similar manner, where water is a driving medium either in a water-drive reservoir or in a secondary recovery operation where water is artificially injected, it is possible to predict the amount of water that has to be handled and the amount of oil that will be produced from time to time from different parts of the field.

Reservoir pressure predictions. In the depletion-type reservoir previously described, the entire future of reservoir pressure can be predicted from a knowledge of the total oil and gas in place and the characteristics of the reservoir oil. At the same time, with some reservoir pressure history and a known volume of oil and gas production, the reservoir mechanics engineer can calculate the total volume of original oil and gas in place without determining pore space volumes as measured by porosity analysis on cores. This in effect is what we refer to as a "material balance." It is simply Boyle's law extended to a two-phase system (gas and oil), and the physical characteristics of the reservoir gas and oil become the key to calculating expansibility data necessary to perform a material balance.

On the other hand, in a water-drive pool, reservoir pressure predictions become a function of the ability of a large area surrounding the reservoir to deliver water into the original oil-saturated portion to replace reservoir fluids produced. The continuity of porosity and permeability, the total volume of the reservoir farther and farther away from the oil-saturated portion, and the expansibility of the fluids within the oil reservoir and those beyond its limits are key factors.

In some reservoirs permeability is so great that only a nominal pressure drop within the oil field is necessary to cause an inflow of water that will replace the fluids withdrawn and thus maintain pressure at some level slightly below the initial reservoir pressure. On the other hand, the distribution of permeability and the volume of the reser-

voir may be such that a pressure drop of several hundred pounds is necessary before enough water will enter the reservoir to replace fluids withdrawn.

It is obvious, then, that the whole problem of reservoir prediction is an involved one and requires "cut-and-try" procedures. Because of this, the reservoir mechanics engineer quite often employs electrical analyzers which involve, within electrical circuits, dimensionally duplicating factors controlling reservoir performance. Thus, after facts have been determined, rapid calculations can be made by varying the factors relative to each other.

Some Specific Applications of Reservoir Mechanics Analogies

Water-drive reservoir performance predictions. The East Texas field is an excellent example of how the reservoir mechanics engineer has evaluated the pressure performance in a reservoir influenced by factors in an aquifer miles away. Figure 3 is a diagrammatic sketch of the East Texas oil field, which is located in a pinch-out in the Woodbine Sand at the eastern extremity of a large Woodbine Sand aquifer in which several other fields are located. One can imagine the problem of the reservoir mechanics engineer when he faces the fact that reservoir performance within the 200-square-mile East Texas field is controlled to a large extent by conditions in an aquifer covering some 10,000 square miles.

The problem of the reservoir mechanics engineer was to determine the effect of withdrawals in the East Texas field on the availability of water from the aquifer for replacing those withdrawals. He had to bear in mind also that, as a pressure sink is created within a field, the sink gradually broadens outward to a lesser and lesser degree until it theoretically reaches the outcrop. The farther the pressure sink broadens out, the less the production that can be taken out of the field with pressure being maintained at any one level. Figure 4 shows a pressure performance curve in East Texas.

As a result of reservoir mechanics studies, it was found necessary to inject gradually increasing proportions of the water produced from the field in order to maintain the rate of oil production and still keep the pressure at the desired level of approximately 1,000 psi. The increasing proportion of water injected to water produced (Fig. 4) shows how these plans have been carried out. East Texas is a fascinating illustration of this type of reservoir mechanics problem, but every water-drive or combination-type field is subject in varying degrees to the same kind of determination.

Predicted performance of a depletion-type reservoir with and without gas injection. Using factors heretofore described, where the oil and gas in place, the relative permeability characteristics of the reservoir rock, and the characteristics of the reservoir oil have been determined, the entire production history with and without gas injection to control the pressure can be predicted. From studies of this nature, the whole depletion program of a reservoir can be outlined, and the economic value of gas injection as a secondary recovery means can be determined. Furthermore, after such predictions have been made, it is possible efficiently to design gas-processing and -injection plants and schedule their construction and operation to meet the requirements of the reservoir mechanics engineer in effecting the greatest degree of recovery by gas injection from the pool being served.

The reservoir mechanics engineer is continually plagued by changes in conditions brought about by market demand and by actions of regulatory bodies whereby rates of production have to be changed,

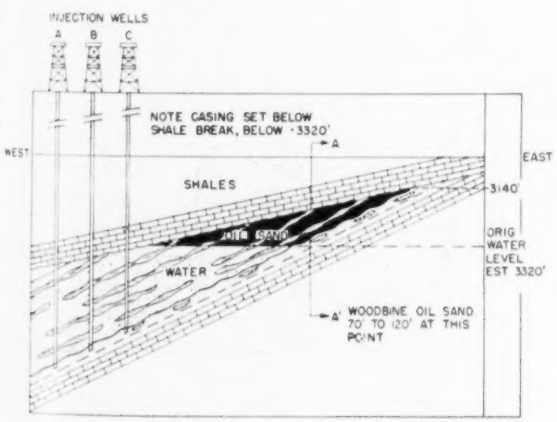


FIG. 3. Generalized west to east cross section, East Texas Field. (*A.P.I.*, 1947, **27**, (4), 37.)

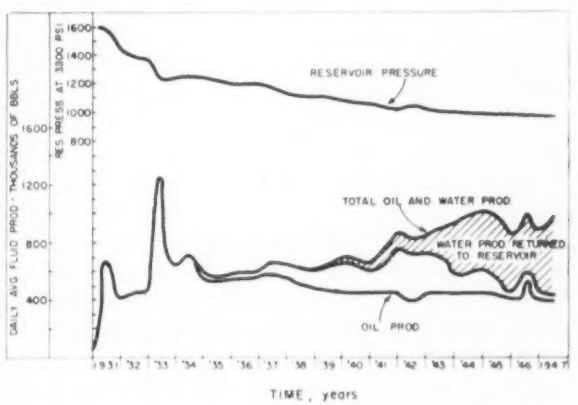


FIG. 4. Performance record, East Texas Field. (*A.P.I.*, 1947, **27**, (4), 42.)

and thus he goes ahead with a complete new set of predictions in order to make adjustments for changes in operating practices. Even so, he has not yet fully determined the significance of certain factors that keep recovery efficiencies down to 75 per cent in unconsolidated water-drive sand reservoirs, 50–75 per cent in some of the consolidated sand water-drive pools, and 15–25 per cent in the depletion-type dolomitic limestone reservoirs.

Surface and interfacial tension factors. As important as his contribution has been to date, the reservoir mechanics engineer has been increasing the recovery of oil while dealing only with that portion of the total oil in place which is *relatively* easy to obtain. As depletion of domestic oil reserves proceeds and as new fields are becoming less and less frequently discovered, he is economically justified in investigating processes for increased recovery of the oil that is more difficult to produce. To explore this region, he must be conscious of the tremendous surface area involved within pore space in a reservoir rock. For example, in one cubic inch of very permeable unconsolidated sand, the area of rock surface in contact with the wetting agent is approximately 1,000 square inches. If the rock is preferentially water-wet, the area of the oil surface in contact with the water surface is somewhat of the order of 500 square inches per cubic inch of reservoir rock. In all sand pools, the reservoir mechanics engineer has to evaluate the surface and interfacial tension forces controlled by this area of contact and their combined resistance to reduction in the thickness of the residual oil film normally left after pore space has been invaded by water.

It is altogether possible that different types of reservoir rock are preferentially oil-wet to some degree and may be completely preferentially oil-wet in some cases. In such an instance the reservoir mechanics engineer must first determine this as a fact and then design some applicable process where, for example, water injected could be chemically treated so as to reverse the characteristics of the rock and make it preferentially water-wet.

Scientific terms such as "contact angles," "surface tension," and "interfacial tension" must strike a familiar note to the chemical engineer; and the science of reservoir mechanics engineering will become more and more infiltrated by the chemical engineer who wants to make a real contribution toward increasing the total reserves of his country's oil pools and the availability of these reserves.

Solid-liquid phase relationships. Another factor that may become really significant in some of our relatively tight depletion-type reservoirs from which paraffin base oils are produced relates to the possible deposition of paraffin or wax in the pore spaces of the reservoir, particularly in close proximity to the penetrating well bore. It is known that paraffin can accumulate on the face of the sand in the well and completely plug it up until the paraffin is removed. Whether small particles of this paraffin material can be deposited in the pore space beyond the face of the sand is much debated. The combination of factors at least invites such a precipitation, namely, "streaming potentials," the reduction of paraffin solubility within the reservoir oil as it loses gas out of solution with decrease in pressure, and the possible cooling effect brought about by the continuous vaporization to which the reservoir oil is subjected as it converges in its flow toward the well. Here again the problem is first determining if such precipitation takes place and, second, designing a mechanism that will prevent it through control of reservoir pressure, or remove it as an obstacle to flow by some other means. This phenomenon may plague the operator in many pools more than he suspects.

Tertiary recovery processes. The reservoir mechanics engineer to date has primarily been concerned with the use of gas and/or water as a displacing medium for stripping oil out of the various types of reservoirs. In certain reservoirs where recovery efficiency by these methods is relatively low and where it can be substantially increased by some means yet to be discovered, the reservoir mechanics engineer must explore other ways of removing such oil economically. At least imaginable are various types of thermal processes whereby a large portion of the oil might be distilled away from its tenacious grasp to other immobile surfaces, such as interstitial water and/or reservoir rock.

The science of reservoir mechanics engineering has been brought about by conservation-minded leaders in the oil industry who have charged the members of their engineering staffs with the responsibility of guaranteeing maximum oil recovery. The place of the reservoir mechanics engineer in the oil industry has become more and more important in the face of a growing demand for oil and the constant realization that, for every billion barrels of oil produced, the nation's natural petroleum resources are reduced by just that volume.

The science of reservoir engineering has gradually evolved from one consisting basically of geology and mechanics and has gradually pulled within its sphere the special sciences of physics, chemical engineering, and thermodynamics. Who knows at this moment to what other fields of specialization the reservoir mechanics engineer may ultimately look for his further refinement?

SCIENCE ON THE MARCH

MAKING THE OCEAN A TEST TUBE

THE rolling waters of the ocean exert a strong appeal to most of us, and since time immemorial men have answered that call in various ways. For many centuries men have gone down to the sea for the purposes of marine exploration, for foreign commerce, for naval warfare, for fishing and bathing. Ocean activity never ceases, and the season never closes on its adventurous appeal.

We do not hear a great deal about sea explorers these days, but the Age of Marine Discovery is by no means over. Many scientists think it is only beginning. New land is scarce, of course, but the search for new scientific discoveries in the ocean is now more active than ever before. Studies are in progress far out at sea, as well as on the continental shelves close to land, where petroleum engineers are searching for oil. Bobbing up and down on the deepest waters one will find scientific expeditions from a number of nations, charting the depths of the sea with echo-sounders, recording the water temperature, and even the color of the daylight at depths down to 100 fathoms or more. These scientists are interested in such things as the chemical composition of the carpet of mud that lies on the ocean bottom, the shape of the profile of the ocean floor, and the gas content of the water at various stages on the way down.

The Ocean a Natural Laboratory

There appears to be no limit to man's curiosity about ocean life. It is a very practical curiosity. What are the movements of marine animals in their watery home? Where do fish go at various periods, and where do they get the vitamins that often make their liver oils so valuable? What new plants and what strange creatures dwell in the deep waters miles down? These are some of the problems being actively investigated today.

With the excellent echo-sounders now available, a moving school of fish can be followed in its travels, and a scientific fisherman can tell beforehand whether the school is large enough to be worth fishing for and how deep he should set his lines and nets. It is even claimed that in some instances the *kind* of fish can be made out with these modern instruments. And by the use of recently developed core-samplers, the plants, animals, and samples of the mud from the bottom can be brought to the surface for examination. The ocean is thus seen to be a vast natural laboratory; and the things that can be learned about its structure, and its inhabitants and their ways, are of great practical importance to every one of us.

One of the important ways in which the ocean laboratory has been put to work is for the general study of corrosion by salt water and salt air, and the effects produced by marine organisms on metals, wood, and other materials. The ceaseless interest of man in ocean activity and the vital importance of marine industries make it essential that we find out the most resistant materials to use for equipment in connection with those industries. Before going down to the sea in ships, or even living close to the shore, it is wise to ascertain what will provide the most lasting resistance to corrosion by ocean water and marine atmospheres.

Marine corrosion, like other kinds of corrosion, is known to be a natural electrochemical phenomenon, the basic principles of which have been established by study and research in many plants and laboratories all over the world. However, the most reliable guides to the proper selection of materials for particular purposes are always based on studies carried out under the actual conditions met in service. This is the basic reason for the research activities conducted by International Nickel at its Kure Beach Testing Station. To provide more complete knowledge of the effects of salt water and salt air upon metals, wood, plastics, painted surfaces, and even rope, the ocean literally has been placed in a test tube.

The station at Kure Beach was established by The International Nickel Company in 1935 for the immediate purpose of comparing the corrosion resistance of low-alloy and carbon steels. Soon other materials were added to the program, so that eventually comparative tests were being made on all kinds of ferrous and nonferrous metals and alloys. The testing facilities have since been extended further to observe the behavior of several kinds of protective coatings—both metallic and organic, including antifouling formulations—the effects of marine growth on wood as well as on metals, and even the results of exposure of ropes to sea spray and sea air. In 1940, facilities for ex-

Aerial view of Kure Beach Corrosion Testing Station.

posing specimens to atmospheric attack were added in cooperation with the Carnegie-Illinois Steel Corporation, The Dow Chemical Company, Magnesium Division, and the Armco Steel Corporation.

The atmospheric test lot is an acre in area and has room for 40 racks, each of which will support 700–900 specimens. At the present time, the number of specimens exposed in the atmospheric tests is about 20,000. This is believed to be the largest number of specimens on test at a single station anywhere in the world. The expense of preparing these specimens for test and caring for them makes the investment in specimens in this test lot amount to well over $200,000. This is probably a conservative estimate, if the full cost of the specimens is included. Many of these represented special materials at the time they were placed in test, involving special melts and other expensive processing. Many new materials developed in this country and abroad during and since the war are now included. The number of specimens now exposed in sea water is about 3,000 and, during the past fourteen years, the number of specimens so tested has been over 15,000.

The sea-water tests originally were located in a channel through which sea water was pumped from the ocean into the bromine plant of the Ethyl-Dow Chemical Company. The closing down of this plant in 1946 made it possible to remove the test structure to the basin formerly used as the intake for the pumps that supplied the water for the plant. In the tests, most specimens are exposed on racks continuously immersed at a depth of from three to four feet.

At the new site, the ocean is literally "put in a test tube," with the effects of the tides and other factors being captured exactly as they exist along the shore line a few feet away, except that the bulkheads break the force of the surf.

Among the advantages provided at the test site are:

1. A continuous supply of full-strength sea water uncontaminated by industrial wastes, oil films, or other

pollution that interferes with tests in harbors or near big cities.
2. A relatively wide range of sea water temperature (45°–85° F.).
3. A long season of growth of a large number of marine organisms (of particular value in studying antifouling alloys and coatings).
4. Protection against the physically destructive effects of storms and high waves.
5. A fluctuation in water level which permits observation of waterline and intermittent immersion effects when desired.
6. Adequate protection to prevent the theft of or tampering with specimens.
7. The availability of personnel and mechanical equipment needed to erect the testing equipment, keep it in repair, and handle the heavy racks of test specimens.

The necessity of providing facilities for the exposure of specimens to attack by sea water and marine atmospheres under natural conditions has been demonstrated by the limited practical usefulness of laboratory tests on relatively small specimens in simple salt solutions, synthetic or reconstituted sea water, or in salt spray boxes. Laboratory tests provide a valuable supplement to tests under natural conditions—particularly in preliminary investigations and in throwing light on the mechanisms of sea-water corrosion and the factors that control it. However, sea water is something more than a mixture of chemicals; it is a living thing, and its corrosive reactions over an extended period can be studied properly only by the exposure of specimens to attack under natural conditions.

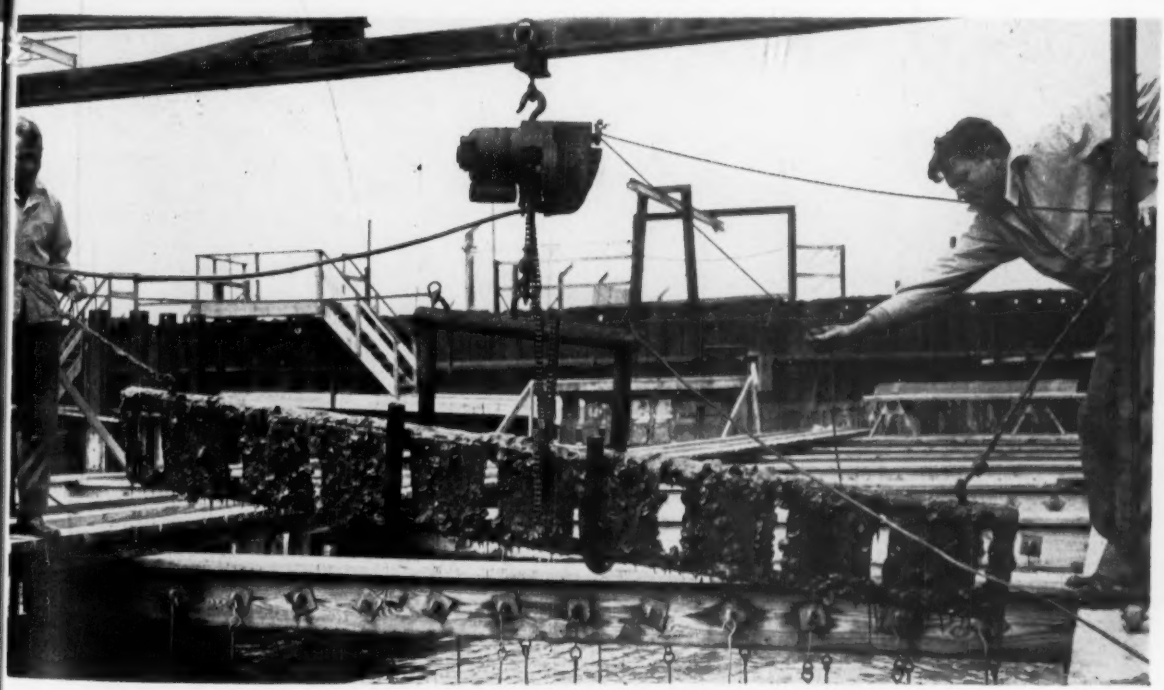
Comparative tests at Kure Beach and in synthetic sea water in the laboratory have shown definitely that laboratory conditions are quite inadequate for measuring the behavior of metals and alloys in natural sea water.

Various Effects Studied

The study of the antifouling characteristics of metals, alloys, plastics, and protective coatings has been an important phase of research during the past few years. This phase of the investigation has been conducted with the active and invaluable cooperation of Professor William F. Clapp, of the William F. Clapp Laboratories.

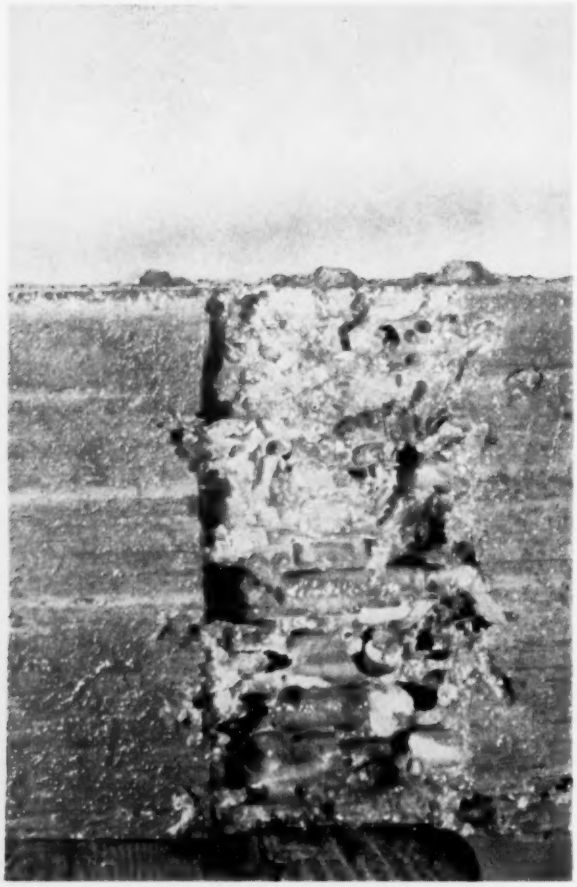
The racks themselves, with the exception of those furnished by individual cooperating companies, are made either of Monel or of an alloy which contains 70 per cent copper and 30 per cent nickel. Racks made of both these alloys are welded with Monel rod, and the supporting rods are Monel. Some of these racks and rods have been in continuous use since 1935 and appear still to be as serviceable as ever.

Specimens are fastened to the racks by means of Monel machine screws. Galvanic effects are prevented by the use of bakelite insulating tubes over



Electric hoist brings up rack of specimens for preliminary inspection, to be followed by detailed laboratory examination of marine growths and effects of corrosion.

the bolt shanks and insulating washers between the specimens and the racks and the Monel washers under the bolt heads. Incidentally, some of the original lot of Monel screws and nuts are still in use and in excellent condition after twelve years. The efficacy of this method of avoiding galvanic action has been demonstrated by the absence of such effects on specimens of materials like magnesium, aluminum, and zinc-coated steel.



Teredos, or "shipworms," cause this kind of destruction in interior of wood blocks.

The period of exposure of a group of specimens may vary from six months (rarely less) to several years—the longest to date, twelve years. The number of specimens exposed varies from about 1,000 to 3,000, depending on the nature of investigations in progress.

The need for more precise information on the abilities of alloys to withstand the severe erosive effects of sea water moving at high velocity has led to the development and use of several types of apparatus for studying such effects. The first of these to be used at Kure Beach is called the "Whirligig." The essential parts of the device are a large Monel disc, a "K" Monel shaft to which it is fastened, and a motor and gears to drive it.

Specimens may be fastened to this disc and whirled through the sea water at velocities up to about 30 feet per second, depending on the type of specimen and where it is placed relative to the center of the disc. All specimens are electrically insulated from the disc and from each other. Condenser tube alloys are tested in the form of short sections of tubes placed in brackets which simulate sections of tube sheets. As the disc revolves, salt water rushes through these tubular test pieces under conditions of great turbulence, resulting in erosive effects as severe as any likely to be encountered in service.

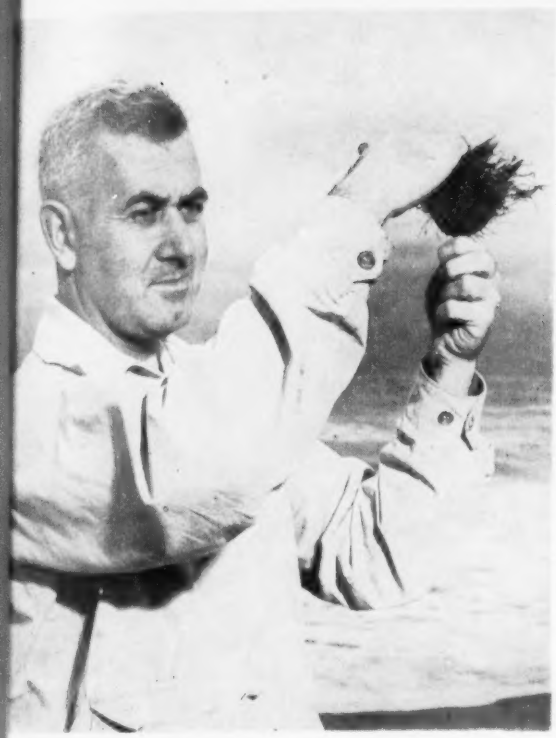
A somewhat similar type of erosion-testing machine makes use of specimen-supporting discs mounted on a horizontal shaft revolving at high speed in a tank through which sea water flows continuously. Tubular specimens may be attached to these discs in the same way as on the "Whirligig" discs, and other specimens in the form of flat strips or bars may be attached so that they project beyond the rims of the discs and cut through the water as the discs revolve.

Another type of apparatus, designed specifically for studying what is called "impingement" attack, subjects submerged specimens to the action of jets of sea water containing air bubbles, which are drawn into the water by an aspirating or injection effect of the jet. This is called the aspirator-type jet-impingement test apparatus.

Still another testing device rotates specimens in the form of discs while they are submerged in flowing sea water. There is a variation in velocity from the center to the periphery of these discs, so that the velocity above which severe erosion is observed can be calculated readily.

A special set-up is used for studying the galvanic potential relationships among metals and alloys that are used together in sea water. Here, the specimens remain stationary in a hard rubber holder in which they form the walls of a narrow channel through which sea water may be passed at velocities up to 15 feet per second. In addition to permitting measurements of potential and current flow, this apparatus is used for studying the polarization characteristics of the metals, of fundamental importance in predicting and explaining their galvanic behavior.

Finally, effects of velocity are studied more directly by setting up model piping systems and experimental condenser tube bundles which will



Author holding billiard ball showing extent of marine growth that has developed after only a few months' exposure in salt water.

duplicate the flow conditions of ordinary service.

These several methods of test reproduce the kinds of attack encountered in service and permit resistance to erosive effects to be measured quantitatively over relatively short periods of time. Since the results for standard materials have been correlated with long-time service experience, it is possible to determine the probable merits of alloy modifications or new compositions in relatively short periods of time.

There is a variety of other types of equipment and apparatus installed at the Kure Beach project to aid in the studies. These include a wooden trough in three sections at three different levels, all connected, through which sea water from the basin is pumped constantly at a velocity of 3 feet per second. The combined length of the three sections is over 300 feet, and the trough has a capacity for 1,000 specimens, depending upon size.

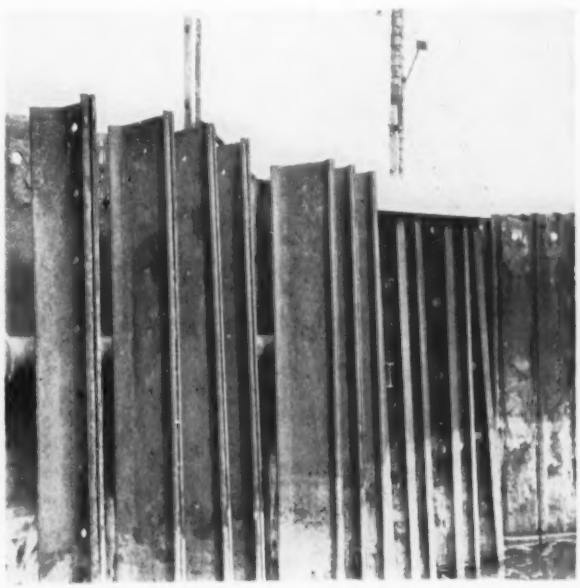
In some cases it has been possible to expose specimens in the identical form and as nearly as possible under the identical conditions of a proposed use. An example is provided by some long-time tests on plain and alloy steels as they might be used for bulkheads or structural steel piling. These specimens are in the form either of interlocking piling sections or I beams.

The progress of corrosion is determined by periodic measurements of the decrease in thickness of the steel. These measurements are made with micrometers around the peripheries of the holes drilled for the purpose at intervals over the exposed length of the specimens. This test has already yielded interesting information about the distribution and extent of corrosion of the steels above and below the water and in the region between high and low tide levels.

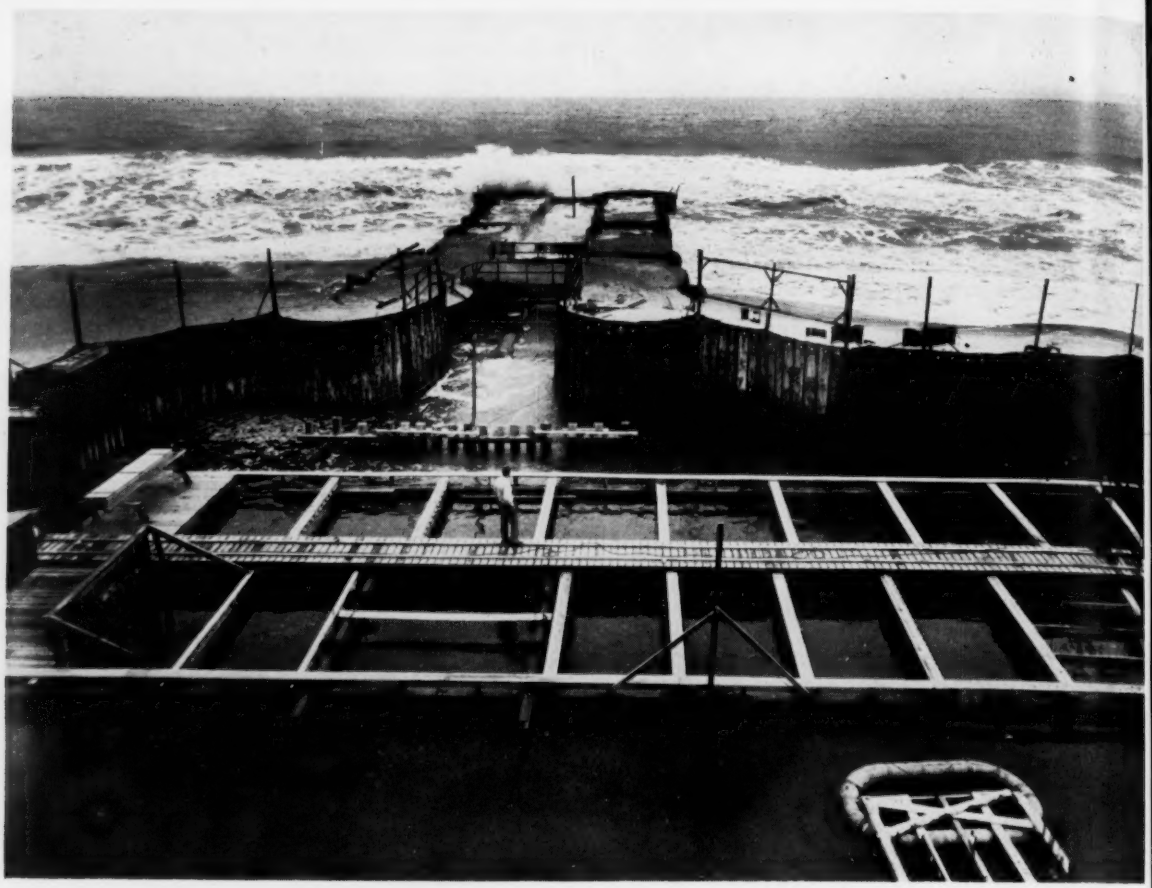
Since corrosion by marine atmospheres is equal in importance to corrosion by salt water, facilities have been provided for extensive atmospheric corrosion tests. At the atmospheric test lot, more than 20,000 specimens are now exposed by Inco and cooperating companies. This lot is located about 250 yards from the ocean shore. The atmospheric racks face south, and the specimen frames are set at a slope of 30° from the horizontal.

Effects of atmospheric corrosion are measured by visual observation and by determinations of weight loss, changes in mechanical properties, or both. The test frames, with their insulators, may be made to accommodate several types and sizes of specimens. Usually enough specimens are exposed originally to permit their withdrawal in groups of from 2 to 5 (most frequently 4) after various time intervals so as to permit observations of changes in corrosion rates with time.

New specimens of certain key materials are put on the racks each time a large group is removed or a new group is installed. These key specimens



Full-scale test specimens of steel piling exposed to corrosion by mud, salt water, and salt spray.



Thousands of specimens in a million gallons of sea water make the Kure Beach basin an oceanic test tube.

provide information on changes in the corrosivity of the atmosphere itself from year to year, which assists in the interpretation of the results of tests made over different time periods. One large group of about 1,500 specimens of different steels was distributed over the racks at random so as to discover any possible effects of rack position on the results obtained. So far, no such effect of position has been observed.

Another important study combining sea water spray as well as sea air is being carried out on a site about 80 feet from the shore, where a stand about 8 feet high and roughly 100 feet long by 15 feet wide has been erected. This stand supports both Monel and wooden racks, some vertical and others at the same 30° angle used for the atmospheric tests. There are about 1,000 samples of materials under examination, including 20 springs typical of those used for railroad cars.

The same conditions obtain here as in the atmospheric lots, except that the air frequently is laden with concentrated spray from the ocean, driven against the specimens by the winds.

Laboratory on Site

One of the most important facilities at Kure Beach is the laboratory located at the site. This provides equipment for such studies as measuring loss in weight of samples, microscopic examinations, and the like. This laboratory checks each specimen, keeps an accurate record of time of exposure, location on racks, and carefully chronicles information on performance. The laboratory also includes a marine museum where sample specimens from the many studies are displayed.

F. L. LAQUE

Development and Research Division
The International Nickel Company, Inc.
New York

BOOK REVIEWS

GENETICS FOR THE ELITE

The Elements of Genetics. C. D. Darlington and K. Mather. 446 pp. Illus. $3.75. Macmillan. New York.

THE title, *The Elements of Genetics,* suggests an elementary text, and the publishers describe the book on the jacket as "a concise textbook." Its contents hardly agree with this description; apart from monographs on special topics, this is perhaps the most "advanced" book on general genetics ever published. To be sure, it does contain brief descriptions of such "elements" as cell division and Mendel's laws. Yet anyone not previously familiar with genetics will emerge from reading it with more awe than enlightenment. The art of lucid and didactically effective presentation is something which eminent scholars do not necessarily possess.

The purpose of the book is stated by the authors in the opening paragraphs of the Preface:

> There are two ways of attempting to describe a part of nature in scientific terms. One is to deal with the area which has been exactly mapped by experiment, with the ensuing generalizations and predictions, and to leave the rest empty. The other is to go further and use our knowledge of the mapped area to fill in the empty spaces according to the more likely assumptions. The first method is evasive, the second is hazardous. We prefer the second, and have adopted it.

The authors have kept their promise, and herein lie the outstanding interest and value of this remarkable book. It is one of the rare works which do not stop at systematization of familiar truths but go on to blaze trails for the exploration of the unknown. It contains more new, brilliant, and challenging ideas than any other single book on genetics or related disciplines.

Only some scientific puritans would cavil at so exciting an undertaking as an attempt of two outstanding creative minds to reconnoiter and catch a glimpse of the "empty spaces" on the map of their science. But one might have wished that the delimitations of the "exactly mapped" and the "filled-in" areas were made clear; in this book they are quite indistinguishable. To take a single example, we are told that human culture is the aggregate of reactions between different genotypes, and that the "contrasts of genetic capacity for culture between different genetic groups" increase with increasing tempos of human change. Now, these are assertions of no trifling significance. Contrary views are entertained by many scientists, and the reader is entitled to the consideration of being given an opportunity to examine some of the evidence on which the assertions rest. The only explanation he gets is that "The existence of a genetic component or racial basis for culture has long been obvious enough to have been worth denying." These dicta are then used as a foundation on which to develop a theory of genetic control of language. The concentrations of the gene for the O blood group in human populations and the distribution of the languages using the "th" sound are shown on two maps of Europe. A similarity of the two maps leads to the conclusion that the phonetics of a language are determined by the genetic speech capacities of the populations that speak that language. All one can say about this theory is that it is most interesting and thought-provoking. Whether it happens also to be correct is for the future to determine, but an unwary reader may come to believe that the genetic controls of culture and language are among the "elements" of genetics.

Ordinary victuals are meant to satisfy the everyday needs of the mass consumer. Choice viands are for connoisseurs (and we need not decide here whether being a connoisseur is due to genes or to education). There exist similar distinctions among scientific books—some are good, and a few are good for scientific gourmets. *The Elements of Genetics,* as expounded by Darlington and Mather, is a book fit for the delectation of experts.

TH. DOBZHANSKY

Columbia University

ROMANTIC CRAFT

The Alchemists. F. Sherwood Taylor. x + 246 pp. Illus. Schuman. New York.

THIS latest addition to "The Life of Science Library" is a readable, revealing, and well-documented account of the origin and evolution of alchemy. The author's style is pleasing, and the text is appropriate to the scientist, although it is not beyond the comprehension of educated laymen interested in this romantic craft. The book is interestingly illustrated.

Dr. Taylor has painstakingly traced the history of alchemy from about 300 B. C. through its own transmutation into the science of chemistry around the seventeenth century. But, beyond this traditional survey, the author has undertaken to defend alchemy against the charge of "black magic" and to show that alchemy was essentially religious. Its philosophy, he says, was . . . "aimed at the unification of all nature in a single scheme, the author of which was avowed to be God."

In the author's view, alchemy was a personal pursuit in contrast to the cooperation of true science, except with respect to laboratory technique. Here alchemy is credited with making significant contributions. The alchemists, in Dr. Taylor's concept, intended to be useful by providing the means for progress in their

own hands but never for use by the public. They feared the disclosure of knowledge because of the probable abuse of the alchemical secret by wicked men. He feels they had a sense of moral responsibility for their work, not always shared by scientists today. In this connection, he cites the accomplishment of the uranium pile—a transmutation placing power in the hands of men not prepared by spiritual training to properly use this knowledge.

The book will be enthusiastically received by scientists. It should be required reading for all students of the physical sciences.

W. Paul Briggs

Pharmaceutical School
George Washington University

ARS LONGA . . .

Profile Art. R. L. Mégroz. xii + 131 pp. Illus. $7.50. Philosophical Library. New York.

From Cave Painting to Comic Strip. Lancelot Hogben. 288 pp. Illus. $5.00. Chanticleer Press. New York.

WHAT comes in sight when we take a long view of the picture-making antics of our species? Probably it is a good thing that the farther we look back the fewer are the objects remaining to comprise the pictures of those bygone eras. Here are the bison, deer, and antlopes done in outlines and ochres on the limestone arches of Spanish caves. They are part of the early chapters of the artist's book of Genesis. So also are the prehistoric "problem pictures" of the Wondjina and wallabies of northwestern Australia. These are focal points from which Professor Lancelot Hogben and Mr. R. L. Mégroz begin their startlingly different books on how the human animal communicates by "thinking and drawing lines around his think."

In *Profile Art* Mégroz looks at the vast scene with eyes half-closed to shut out trivia—a good painter's technique. What he sees is man and his world represented in variations of the silhouette: sometimes basic and symbolic, suggesting much more than it ever states; at other times and places degenerating to its least potential, the filled-in frivolous shadow. These outlines are found glorifying the shoulder of the amphora, the Phoenician coin, the mandarin robe, the French tapestry, the Victorian fire screen, the ballet backdrop. The painted or cut profile is versatile and can be anything from cut paper to cut stone, from a crochet pattern to a marble frieze. Man, says Mégroz, is afraid of the fleetness and insecurity of the world and so is intent on trapping the shadows in order to hold the moments that he loves and give them an aspect of endurance These shadows are as tragic or frivolous, as powerful or weak, as the mind that transcribes the outline. The book demonstrates tacitly a point it never makes—there is no neat demarcation between an outline and a molded, "real" object. A flick of the magic line can give the silhouette solidity and fluency. Where and what is the "outline" when one draws? It is the line that tells the most. It may leap anywhere, like a flame; it may fuse objects or divide them. It is the sure and unhampered "moving finger" that records the way we inhabit the world.

Mégroz' text is simple chronological description of many phases of the use of silhouette, written in a style indicative of considerable ethnological research but easily within the general reader's grasp. Each chapter has drawings showing types of patterns and how they were used—on cave walls, copper pots, tiles, and tapestries. Considerable space is given to the period 150 years ago, the heyday of silhouette portraiture. Modern textiles and some humorous cutouts are included. Altogether the 200 distinctive pictures and plates in *Profile Art* are well chosen both to beautify and to illustrate, and constitute "a mine of constructive suggestions to designers in every medium."

Hogben's *From Cave Painting to Comic Strip* (subtitled "A Kaleidoscope of Human Communication") contains enough excellent material for half a dozen books, but as *one* book it reminds us of Stephen Leacock's Lord Ronald, who "flung himself upon his horse and rode madly off in all directions." It is in many ways a fine book, reflecting the author's enormous knowledge and his amazing ability to make lucid what he knows, but it seems to lack viewpoint. Hogben seems to try to write the history of art, of illustration, and of the printed word in one volume. There are excellent chapters on symbols, hieroglyphics, the calendar, numerals and mathematics, and on all the techniques of mass production of visual communicative media, tracing the evolution of the tools involved in visual education. Early in the book the author describes it as a "picture-book about picture-making," and so, when he includes a chapter, however interesting, on the invention and use of the cipher in mathematics and another on the internationalism of free speech (including a denunciation of modern educational methods), we wonder, like Alice, how we got from the cucumber frame to the Mad Hatter's party. The illustrations (230 of them) were selected by Marie Neurath, and she did a fine job staying within yoohooing distance of Hogben. They are pertinent, varied, and often beautiful.

But Mr. Hogben, having traversed the historic field of illustration as an educating method, comes to the close of his book with some concrete suggestions about what social applications can and should be made. As is often stated, man is heading toward more and more learning about less and less. This rightly is of concern to Hogben. He fears there will be a breakdown of bonds of understanding among peoples. General education, he says, can and should be speeded up in the cause of international understanding. He stresses the use of a modern tool developed by the Isotype Institute of London and the late Otto Neurath as likely to help greatly in bringing about a literate, understanding world populace. The tool is a system of standardized printed symbols used with great effect in statistical and factual illustration and, according to Hogben, not yet fully exploited. These are the visual aids that could make possible speedily a universal *used* secondary lan-

guage. This speed-up of general education is necessary, he thinks, because the body of scientific knowledge is so great and so increasing. The aim is the attainment of world government, world harmony, world peace. We believe that the widespread use of these visual aids to factual education would help to fulfill the hope for peace, and Hogben's efforts are praiseworthy and deserving of support. But we could ask, Can we ever assimilate this snowball of facts as fast as it rolls up? Will the individual break, mentally and physically, under the strain of the speed-up? Isn't it a slowing-up we need instead of acceleration? Or at least some sort of a reversal of emphasis such as that suggested by Lewis Mumford in his address at the Centennial Meeting of the AAAS in 1948: "We must reverse the order of development which first produced the machine; we must now explore the world of history, culture, organic life, human development, as we once explored the nonliving world of nature. We must understand the organics of personality as we first understood the statics and mechanics of physical processes; we must center attention on quality, value, pattern, and purpose, as we once centered attention on quantity, on physical relationships, on mass and motion."

GRACE E. OEHSER

Arlington, Virginia

NATURE IN HER VISIBLE FORMS

The Sea Shore. C. M. Yonge. xvi + 311 pp. Illus. 21 *s*. Collins. London.

AT LAST comes a book about the seashore in which an expert has presented a well-balanced account of the many intricate relationships that exist between environment and intertidal marine life. To fulfill such a mission in 300 pages is doubly difficult today, for the author must not only be familiar with the numerous biological principles at work, and have at least a nodding acquaintance with nearly all members of the fauna and flora, but he must have the courage to digest and utilize the vast amount of recent literature on experimental biology and marine ecology. C. M. Yonge's twenty-five years as a professional marine biologist and lifelong role as a lover of the seas have qualified him for such a task.

The book is almost entirely about the animals and plants of the seashores of the British Isles, but, as most American biologists know, the fundamental story of life, death, struggle, and selection is equally applicable to the inhabitants of our own shores. In fact, many of the details are as New England as a Cape Cod clambake. Yonge's seashore, defined by him "as the region between extreme high and extreme low spring tides," is a narrow strip of land probably smaller in area than the intertidal zones from Maine to Virginia. In addition to having a relatively small collecting ground, the British have had the advantage of long since having passed the era of discovering and describing their fauna. With a large public interest in natural history and a large, zealous band of professionals, it is natural that books such as this should appear not infrequently. The early history of British marine biology and an interesting account of the eminent naturalists, such as Pennant and Gosse, who popularized British shore life, are included in one of the first chapters.

To read Yonge's book is to take a pleasant course in marine ecology, not from the blackboard or the pickle jar, but from the salty edge of the tide pool and the shimmering mud flats of the bay. By the sixth chapter, the reader has been introduced to, or has had a well-selected review of, the inhabitants of the seashore, the basic principles of oceanography, and the dynamics, both physical and biological, of the tidal zones. From here, Yonge proceeds with the theme of the book—the story of the close associations of plants, animals, and environment, and how these many forces have brought about limitless variety of specialization in form and habit. An ecological sequence is followed throughout the book, from chapters on Life in Rock Pools and The Intimate Fauna of Weed and Rock to those dealing with Life on Sandy Shores and Muddy Shores. His discussion of Distribution and Fluctuations of the Shore Populations, although brief, is especially interesting, for he shows how the single factor of water temperature can determine the composition of an island fauna and can be responsible for the appearance or temporary disappearance of various species. The prime influence on the marine population of the British Isles comes from the warm waters of the North Atlantic Drift, ultimately derived from those of the Gulf Stream. This, with the influence from the arctic waters, produces an oddly mixed fauna of boreal (or north temperate), arctic-boreal, and lusitanian (or mediterranean) species. American students of mollusks who associate such genera as *Octopus, Trivia* (a cowrie), *Lima* (a file clam), and *Onchidella* (an air-breathing sea slug) with Florida and Bermuda are always surprised to find these genera in "chilly" British waters.

It is difficult to review the illustrations in *The Sea Shore* without being guilty of writing an advertisement for the jacket of the book. The sixty-one color photographs are undoubtedly the finest on marine life to have appeared in print. All were taken from living specimens. The pictures of the expanding hydroids and sea anemones, the sea slug laying its ribbon of eggs, the tentacle-lashing octopus, and the swimming file clam displaying its orange filaments are photographic works of art. There are also nearly ninety carefully selected and uniformly penned line drawings which are adapted from outstanding contributions to marine biology by such workers as Moore, Kitching, Bassindale, and Lebour. The credit lines to these illustrations are the only specific bibliographic references in the book, except for a number of valuable leads included in a special appendix. It is the loss of our reading public that the conditions existing in the American color-printing world prohibit similar low-cost publication of colored scientific photographs.

American biologists had the pleasure of meeting the charming and enthusiastic author of this book during his recent half-year visit to the United States, where he taught and studied at the University of California. As a professional biologist, C. M. Yonge "teethed" on the Great Barrier Reef of Australia in 1928–29 when he was leader of the expedition that established a marine biological laboratory there. He is now regius professor of zoology in the University of Glasgow and chairman of the council for the Scottish Marine Biological Association. Let us hope that he soon returns to lift his pen again to the American seashore scene.

R. Tucker Abbott

U. S. National Museum
Washington, D. C.

Snakes. Herbert S. Zim. 66 pp. $2.00. Morrow. New York.

THIS may have been written for children, but, aside from herpetologists, there is not an adult in a thousand who will not learn facts of value while "borrowing" it from the rightful owner! With an avoidance of technical terms, all the facts about U. S. snakes are arrestingly covered without a misstatement. Each fact is charmingly illustrated by James Gordon Irving in such a manner that they are fixed upon the mind of the reader—or the read-to. The book is a valuable step toward sensible conservation. No better present could be given to a youngster with a flair for natural history.

Chapman Grant

San Diego, California

American Spiders. Willis J. Gertsch. xiii + 285 pp. Illus. $6.95. Van Nostrand. New York.

THIS volume, one of the "New Illustrated Naturalist" series, is on spiders, those small animals which seem to be everywhere disliked and persecuted by the members of the human race. In spite of their numbers (estimates runs from 2,265,000 individuals per acre of undisturbed grassland in England to 11,000 per acre in woodland and 64,000 per acre in meadow near Washington, D. C.), few people see more than a few dozen kinds in their lifetimes. Most of these are the species that live in or around houses.

After the reader has finished the generalized chapters on the life of the spider, spinning, courtship, and mating, he will be prepared to enjoy the specialized chapters on the various major groups of spiders. These are followed by a discussion of the economic and medical importance of spiders. (Perhaps this last is the chapter that will be read first.) The black widow has been so widely publicized that it is pleasant to learn that she is not quite so dangerous as advertised (just the same, don't handle her carelessly!) and that we need not fear other North American spiders. It is plain from the account that courtship between spiders is a hazardous affair for the male, for he is often eventually killed and consumed by the female.

The illustrations are remarkable. There are in all sixty-four plates, half of which are in full color and beautifully reproduced. Unfortunately, the references in the text to the plates are not always correctly numbered. This reviewer thinks that it was a mistake to number the two sets of plates separately, one set in Arabic and the other in Roman, so that there is a Plate 16 and also a Plate XVI. Further, there should perhaps have been a little more discussion of the enemies of spiders.

Edward A. Chapin

U. S. National Museum
Washington, D. C.

PROGRESS OF SCIENCE

Farmer Victorious. William J. Hale. xii + 241 pp. $3.00. Coward-McCann. New York.

DR. William J. Hale, who coined the word "chemurgy" in 1933, presents further additions to the language in *Farmer Victorious.* One is "edurgy," which he defines as "working with or for an edible." Another is "sation," which he proposes "to designate a people born to self-sufficiency."

Dr. Hale suggests that four "sations" will eventually embrace all the earth. The Mongolian would include Japan, China, and the Malay regions. The Slavic would cover Russia, Siberia, "and a deep southern domain to the ocean;" France, Germany, Italy, and much of Africa would fall to the Latin-Teutonic sation. The Anglo-American would wrap up the rest of the world.

Financiers, economists, and international trading are vigorously lashed throughout the first half of the book. Among the author's several highly interesting proposals is an idea for tying the interest rate to the rate of employment, requiring the interest to be decreased by quarterly or half-yearly intervals when employment declines, and permitting it to rise accordingly when jobs become more numerous.

The latter sections of *Farmer Victorious* explore a variety of ideas regarding diet and disease, and develop Dr. Hale's pioneer belief that the annually renewable and inexhaustible resources of the vegetable kingdom can, when scientifically exploited to the fullest, greatly enrich the human race. Readers may not share his urgency to form a new kind of agrarian political party to hurry chemurgic progress along. However, Dr. Hale has never waited for agreement, and no reader will put his book down without feeling that he has been dared to do some new thinking.

Wheeler McMillan

Pathfinder
Philadelphia

A Short History of Science and Scientific Thought. F. Sherwood Taylor. 368 pp. Illus. $5.00. Norton. New York.

THIS book is a notable addition to the numerous histories of science that have appeared in recent years. It covers the development of science from its

beginnings in India, Babylonia, Egypt, and Greece through its eclipse during the Middle Ages in Europe to its reawakening with the Renaissance and its rapid development in recent centuries. The author discusses the progress of physics, chemistry, astronomy, biology, and medicine, and their contributions to human living conditions; in his concluding chapter on The Functions of Science he brings in the ethical and sociological aspects of this progress. The book covers a broad field and does it remarably well.

An outstanding feature of the volume is the large portion of it devoted to reproductions of the writings of scientific workers, ancient and modern. The reviewer was particularly interested in quotations from the writings of certain Greek geometers who laid aside the Euclidean limitation of "ruler and compass" constructions, and succeeded in obtaining solutions for the problems of trisecting an angle and duplicating a cube. In the discussion of the latter problem it should be mentioned that in the mathematical formulas printed in this book there are two typographical errors which, however, are so obvious as to cause little trouble to the reader. There is another rather amusing typographical error in the discussion of Brownian motion on page 307, where it is stated that Brown's experimental work was done in 1837, but was published nine years earlier —in 1828!

On the whole the book is well written and gives an excellent reproduction of the drama of scientific development. The chapter entitled Man Learns the Past is particularly good in pointing out that science is cumulative; that everything that each age discovers remains for all ages, and that consequently man is receiving an ever-greater birthright of power, but little or no increased judgment in its use. The spectacle of the world today is the clearest demonstration of this danger. If civilization is to survive it must discover, adopt, and enforce an absolute ethical standard.

PAUL R. HEYL

Washington, D. C.

CREDO

This I Do Believe. David E. Lilienthal. cciii + 208 pp. $2.50. Harper. New York.

THE problem of effective social action is at least twofold. First, we must define our goals as clearly as possible. This is not easy, but it can be done. Second, we must apply the ideals to specific cases and specific modes of action. This is often much more difficult than the first part. But if we do the first without the second we are impractical idealists, trafficking in glittering generalities; and if we do the second without the first we are busybodies and bunglers, urging change but getting nowhere.

A careful reader of the confession of democratic faith by the recently resigned chairman of the Atomic Energy Commission will recognize immediately that we have in this book not merely an accurate and comprehensive statement of the ideals of democracy, proclaimed with the fervor of one who actually believes in them, but concrete proposals for attaining them—methods which in some cases are novel and promising but untried, and in others have had demonstrated success in the working of TVA.

On the side of the definition of the democratic goal, Lilienthal makes clear that this must be done basically in terms of the freedom and dignity of the individual. Democracy is fundamentally ethical and spiritual. But its glorification of the individual is not the philosophy of "take care of number one;" it expresses itself rather in the outlook which demands that we be an active, living part of the times. Thus the spirit of cooperative action prevails, and in this man realizes his importance. He wants to "know that there are some things that he decides, or has a part in deciding, and that he is a needed and useful part of something far bigger than he is." Democracy makes this possible in a way in which no other form of government does.

On the side of the concrete proposals for the realization of this goal Lilienthal is equally clear. The most striking of these is the suggestion that every educated person who has the proper qualifications plan to devote a portion of his life to the rendering of service in some branch of the government, local, state, or Federal. Such a proposal is designed to overcome the limitations inherent in the position of the man who, having decided on a life of public service, finds that he must make continual concessions to his conscience in order to remain in office. Another suggestion—not so novel, but still relatively untried—is that the common man endeavor to acquire such knowledge on scientific and engineering matters as will provide him with a basis for judgments of policy. This will require him, for example, to abandon what Lilienthal calls the "Myth of the Atom Bomb"—namely, that atomic energy is useful only as a bomb and is nothing else—and will convince him that the development of atomic energy should be freed as much as possible from governmental control and allowed to take place under the tried and true method of individual initiative.

Many Americans seem to believe that greater and greater centralization is the only solution to the problem of the intricacies and complexities of modern technological society. Nothing, says Lilienthal, could be farther from the truth. Effective organization can be achieved with a minimum of central control. Here he is not obliged to rest his case on the power of words; his chairmanship of the TVA has demonstrated his thesis in a much more effective manner.

The book is recommended reading for all those who believe that the first step in going any place is deciding definitely where you want to go.

A. CORNELIUS BENJAMIN

Department of Philosophy
University of Missouri

CORRESPONDENCE

PROBABILITIES

Dr. Weaver's letter (October 1949) commenting upon my article, "ESP—Fact or Fancy?" (August 1949) may be thought to point out a weakness in the evidence for extrasensory perception. Readers should be warned that if ESP is to be rejected, it cannot be upon these grounds.

The essence of Dr. Weaver's article "Probability, Rarity, Interest, and Surprise" (December 1948) is that many events are improbable and interesting, but not surprising. For example, there are so many possible card arrangements that *every* hand in a bridge game is improbable. No one is surprised to receive an ordinary bridge hand even though that particular hand is rare. By the same token, perhaps no one should be surprised to receive a perfect hand of 13 spades. To be surprising, a rare event must also be distinguished, i.e., must fall into a class which has a small probability compared to the probability of other classes. A perfect hand might be classified in several ways: (1) as one of all possible hands (not surprising); or (2) as a perfect hand among imperfect hands (surprising); or (3) as having a certain degree of desirability among hands whose desirability varies (surprising). The last classification avoids a dichotomy of events and allows for varying degrees of surprise in the same experiment. The important feature of this discussion is that whether a perfect hand at bridge is surprising depends upon the point of view. It is not legitimate to adjust one's point of view to suit the data gathered.

The foregoing ideas are well known, although often forgotten. How do they apply to the card scores in ESP? To conserve space, I shall assume that Dr. Weaver is not referring to the optional stopping counter-hypothesis, to the question of a priori *vs.* empirical probabilities, to the nature of the distribution curve, to the possible dilution of spectacular experiments by many unsuccessful experiments, nor to any of the other statistical questions that were fully discussed in the literature of ESP ten years ago. I shall consider the question that I understand Dr. Weaver to have raised: Can the particular ESP scores to which he refers be properly regarded as interesting but not surprising?

In the rigorous application of statistics it is customary to avoid all question as to whether a particular effect can be classified as distinguished, by stating *in advance* what type of statistical analysis will be used and what effect will be sought. In the three experiments cited by Dr. Weaver the prestated purpose was to obtain as high a score as possible. The probability figures to whose mention Dr. Weaver takes exception are probabilities that scores as good as, *or better than,* those actually obtained would occur by chance. This method of expressing a priori probability as the area under the distribution curve, lying beneath and beyond the attained score, is universally followed by scientists in all fields. To say that a probability figure calculated under these circumstances is "irrelevant" is to deny the validity of the statistical method. This can hardly be Dr. Weaver's intention.

There *is* a sense in which a very small probability such as 10^{-20} is misleading. This figure is based upon the statistical data and assumes the absolute honesty, competence, and sanity of the experimenter. In a world without absolutes, one must allow a possible margin for human error. The final "probability" to be associated with any one experiment is always somewhat more than the calculated value—how much more is beyond estimation unless one has intimate personal knowledge of the experimenter.

In an ordinary biometrical experiment in which the data may yield a chance probability of 0.01 or 0.001, the "probability" increment which is needed for experimenter error could well be negligible. In such a case the statistical probability would accurately represent the dependability of the statistical conclusion of the experiment.

When the data yield a chance probability of 10^{-20}, the additional "probability" of human error will certainly overwhelm the chance probability in even the simplest experiment. Hence, the final "probability" to be associated with the experiment will be much more than 10^{-20}.

In fairness to the lay reader I should perhaps have stated that the conclusive odds which (in my opinion) favor the existence of ESP derive, not from the work of any one experimenter, but from the combined results of many experimenters.

R. A. McConnell

Department of Physics
University of Pittsburgh

Professor McConnell has written a very friendly and interesting letter. But I think he has quite missed the real point of my earlier note, and accordingly I judge most of his present letter again to be, like the earlier remarks of his which I criticized before, irrelevant. I reply at a little length, because I really think that the questions involved are both important and of frequent occurrence.

Before proceeding to the main point, let me dispose of any implication that it was my purpose to attack the evidence for extrasensory perception. I explicitly stated in my previous note that *"Absolutely independently of whether the experiments in question are or are not significant"* I wanted only to comment on the correctness of some of Professor McConnell's statements about the experiments. As a matter of fact, as long as twelve years ago, Professor Rhine and I discussed together how it might be possible to get some persons to stop wasting time attacking certain actually

unassailable statistical aspects of his work, and how to get the scientific community to direct its skill and energy to those phases of the problem that deserved and required attention.

I think, in fact, that extrasensory perception is sufficiently interesting and significant so that it is important that it does not gain any unsound and unstable support based on inaccurate statements.

Thus it should be clear—but must be emphasized anew—that I was not criticizing what Professor McConnell *might* have said about ESP. I was criticizing what he *did* say.

Now what is it that he did say? About one set of trials with the Rhine cards, Professor McConnell said "A total of 1,850 trials was made under these conditions. The expected mean score was 5 out of 25. The actual mean score was 7.53 out of 25. This deviation is 10.8 times the standard deviation. The probability of such a score by chance alone is less than 10^{-20}." About a second set he said: "Fifty trials were made per evening over a period from December 27, 1936, to April 1937, until a total of 1,850 trials had been reached. The average score was 18.24 out of 25. There is no need to apply statistics to such a score to recognize its extra-chance character." About a third he said: "The total number of trials, 250, was fixed in advance. . . . The average number of hits was 9.3 per 25. This score gives a deviation 6.8 times the standard deviation. The probability that this might occur by chance is about 0.000,000,000,064." In the last instance the reader is obviously expected to look at this small probability, gasp in surprise, and say "This can't be chance."

Nowhere in Professor McConnell's original article is there any mention whatsoever that the figures he quotes are probabilities of obtaining, by chance, scores as good as, *or better than,* those actually obtained. In fact, Professor McConnell explicitly stated otherwise. He said, for instance, "the actual mean score was 7.53 out of 25. . . . The probability of *such a score* by chance alone is. . . ."

In other words, Professor McConnell said—and I think this is completely accurate paraphrasing—an experiment, consisting of a series of trials, is held; a result occurs; the calculated probability of this specific result's occurring by chance is a very small number; therefore any sensible person concludes at once that something other than chance has operated.

This, I claimed and continue to claim, is a fundamentally incorrect set of remarks.

What I have said so far concludes my defense of my original note to Professor McConnell. But now, as a brand-new topic, I should like to comment briefly on five issues raised by Professor McConnell's letter.

1. Professor McConnell's second paragraph I believe to be correct in general tone, but not accurate in detail. It is not correct that an event is surprising provided only that it falls "into a class which has a small probability compared to the probability of other classes." If an experiment can come out in four ways, with probabilities of 0.99, 0.0025, 0.0025, and 0.0025, then any one of the last three outcomes is surprising (it has a surprise index of 392, which anyone will admit is substantially large compared with 1); but no one of the last three outcomes falls in a class which in a simple unambiguous sense has a small probability compared to the probability "of other classes." It does not satisfactorily patch up Professor McConnell's sentence to insert a few extra words—as, for example, to make it read "of some other classes," or "of at least one other class." The way to tell, I submit, whether a probability is surprising is to compute its surprise index.

2. Professor McConnell says: "I shall consider the question that I understand Dr. Weaver to have raised: Can the particular ESP scores to which he refers be properly regarded as interesting but not surprising?" I hope that I have made clear above that Professor McConnell is quite mistaken in considering that I raised that question. On the contrary, I only asked: In his original paper did Professor McConnell make accurate, convincing, and relevant remarks about the three probabilities in question? I did not "take exception" to the three probability figures he quoted. I took exception to what Professor McConnell said about these three figures.

3. Professor McConnell states in his letter that "In the three experiments cited . . . the prestated purpose was to obtain as high a score as possible." This strikes me as a very surprising statement. I would suppose that any truly scientific experiment would have as its purpose to determine the facts, whether these facts correspond to low scores, high scores, or whatever.

4. Professor McConnell may possibly want to say that in a popular article one cannot be expected to go into all technical details. Of course one can't. But in a popular exposition one has a very special duty not to use wrong arguments. A highly qualified technical audience would be equipped to determine whether the wrong argument nevertheless leads to correct results. A popular audience is not always so equipped.

5. Let us suppose that at this juncture Professor McConnell says: "Well, O. K. Maybe I didn't say just what I should have, or just what I intended to. But isn't it, now honestly, quite clear that these probabilities *are* surprising, are so low that any reasonable person would know that the result did not occur through mere chance?"

I can't properly offer myself as an example of a reasonable person; but all I can say is that I answer Professor McConnell's question by saying, "I strongly suspect that the probabilities in question are surprising; but this fact is not clear to me as yet."

Why not? Well, for several reasons. With respect to one of the three sets of trials Professor McConnell explicitly states that the number of trials *was fixed in advance.* Do we infer that it was not fixed in advance for the other two sets of trials? If not, when were these other two sets of trials terminated? Does the criterion for stopping have anything to do with the outcome to date? Thus a great deal of precise care must be taken to assure the validity of results such as these, prior to